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TECHNOLOGY FOR NATIONAL SECURITY

Report by the Working Group
on Technology, submitted to the
Commission on Integrated
Long-Term Strategy

October 1988

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The Report of the Commission on Integrated Long-Term Strategy, Discriminate Deterrence, was published in January 1988 and is available for sale by the Superintendent of Documents, US Government Printing Office, Washington, DC 20402 for $6.50.

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COMMISSION ON INTEGRATED LONG-TERM STRATEGY

October 21, 1988

MEMORANDUM FOR

THE COMMISSION ON INTEGRATED LONG-TERM STRATEGY

The Working Group on Technology is pleased to present to the Commission on Integrated Long-Term Strategy our report on Technology for National Security.

The report is a product of over one year of research, analysis, and drafting by Working Group members. Our report is consonant with the Commission's report, Discriminate Deterrence, which made substantial use of our preliminary findings and conclusions. Our report provides more comprehensive and detailed information underlying the Commission's report. However, this report is the responsibility of its authors, and the Commission does not necessarily subscribe to all of its details.

The lead author of the Working Group report is Dr. Charles Herzfeld. Other members of the Working Group are Mr. Paul Baran, Mr. Richard Brody, Mr. Thomas Evans, Dr. Robert Frosch, Dr. Robert Hermann, Dr. Donald Hicks, Mr. Anthony Iorillo, Mr. Paul Kozemchak, Mr. Kent Kresa, Dr. Stephen Lukasik, Dr. J. Luquire, Dr. Hans Mark, Dr. J. J. Martin, Dr. John McDonald, Dr. Robert Turner, MGen Jasper Welch, USAF (ret), and Dr. Albert Wheelon.

Government advisors included Dr. Marvin Atkins, Dr. Craig Fields, Dr. William Graham, Dr. John Mansfield, Dr. Thomas Rona, and Dr. James Tegnelia.

Special thanks are due Dr. Mark, who wrote most of the material on the basic physical sciences. Dr. Rona commented in detail on several versions of this report. Dr. Fields met with and briefed us often and helped make accessible important material. Dr. William Bajusz deserves thanks for his outstanding help with the final editing.

Special thanks are due to the members of the Commission who have been very helpful to and encouraging about this effort. Particularly General Bernard Schriever should be mentioned, to whom this work in a very real sense is dedicated.

Charles M Herzfeld

Charles Herzfeld
Chairman
Technology Working Group

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MAIN POINTS

For 40 years, U.S. and allied national security policies have been predicated on credible deterrence of nuclear and non-nuclear war with the Soviet Union and its allies. Underlying this has been the realization that only through qualitative superiority would we be able to deter attack, or defeat it if deterrence failed. As a consequence, this country established a unique process, born in World War II, for supplying our troops with the best equipment that could be made available. Thirty to forty years ago, this process--which integrates scientific discoveries, technological inventions, understanding of military operations, and industrial know-how--functioned well and produced extraordinary weapons systems (the B-52, Minuteman, Polaris, U-2, satellites, and so on). This process, in a very real sense, was an American invention and was the comfort of our allies and the envy of our adversaries.

All this has begun to change. Over the last 20 years, we have seen a gradual weakening of this marvelous process. Imperceptible at first and so gradual that the seriousness of the change is only barely apparent now, and some still do not see the danger and prefer "business as usual". We are seeing a steady erosion of the commitment to qualitative superiority. We have seen declining funding for the defense technology base, reduced willingness to take calculated risks to advance the state of the art, explosive growth of a highly destructive, adversarial process at work between the Department of Defense (DoD) and defense industry, and a dramatic increase in Congressional micromanagement of defense programs. These trends must be reversed if qualitative superiority is to be saved.

In the days of a drumfire of stories about $500 hammers and indictments of industrial and Government people on charges of fraud, it may seem quixotic to call for better, more cooperative relations among all in the DoD procurement process. This Working Group has no illusions that this will be easy to establish, but feels there is no real alternative. To gain more productive relationships, the following actions must be taken:

- First, the highest legal standards must be upheld and violators prosecuted to the full extent of the law, else confidence in the very processes of Government will be undermined.

- Second, the total weapon acquisition process must be reformed and rejuvenated, otherwise it will sink ever more deeply into a morass of mediocrity, inefficiency, and politics.

What needs to be done immediately is clear. Qualitative military superiority requires a few critical things:

- A long-term strategy that specifies what we wish to achieve in this dangerous world, how we plan to do this, and the resources we must devote to the effort. This strategy is provided by the report of the Commission on Integrated Long-Term Strategy.

- A steady flow of the raw materials from which weapons are made: excellent science and engineering, and innovative military concepts for systems and operations. These require an atmosphere in which innovation and creativity are welcome, can flourish, and are rewarded.

- A clear and realistic view of necessary military capabilities under different contingencies, and how we would employ these.

- An efficient mechanism for turning technical and military concepts into weapon systems that meet identified needs, or else promise to afford major new military capabilities.

- A steady dedication of adequate resources to long-term projects, as well as application of resources for quick reaction to various unusual contingencies.

For the last 20 years, we have been slipping in all the facets of this process. This has been caused by many factors, and all parties to the process have earned their share of blame. Major deficiencies are:

- The technology base is rusting. Though support of basic science has effectively kept up with national growth and inflation, the resources devoted to defense-related technology have been shrinking since the late 1960s.

- Concomitant with the drop in funding for advanced technical work has been a profound change in the atmosphere in which this work is done. This has degraded from the highly innovative style of the 1950s and 1960s to a highly risk averse approach. Daring new technical concepts are now easily defeated by the bureaucracy. No surer way could be found to impose long-term technical stagnation on our military capabilities.

- Recent cutbacks in DoD budgets have begun to undermine the integrity of the entire planning and budgeting process. Today, more than ever before, cost estimates at every level are biased toward low figures, to give programs a chance. All this adds up to unsustainable expectations, which will surely undermine the process even more.

- In part because of these budget problems (but also fueled by other factors), a highly emotional and confrontational atmosphere between the Government and the defense industry has emerged. It is the view of the authors that in 35 years of participating in this process (or observing it), <u>never</u> has such a poisoned atmosphere existed or so much harm been done. Mutual distrust, fear, anger, and frustration are the order of the day.

What needs to be done? These steps must be taken in order of priority and importance:

- A bipartisan working consensus between Congress and the Executive Branch covering clear national security objectives and management approaches must be developed clearly, forcefully, and persistently.

- Part of the consensus must include a thorough revision of the budget process. Realistic budget levels must be agreed to; stability in plans and funding re-established. Microscopic examination of budgets must be curtailed by restructuring budgets into fewer, larger, and functionally meaningful items, and 2-year funding commitments must be instituted. The connection between major budget items and strategic objectives should be laid out more clearly, both by the Congress

and the Defense Department. All partners to this consensus must be held accountable for their commitments.

- Science and technology funding should be increased. If total Research, Development, Testing, and Engineering (RDT&E) funding grows in coming years, science and technology funding should be at a rate of 2 to 3 percent above the rate of total RDT&E growth. If total RDT&E remains constant or even declines, the science and technology accounts should still grow, at a rate of about 5 percent. In either event, the authors would envision science and technology funding growing until it totals around 17 percent of RDT&E or 7 billion in FY 1989 dollars, whichever is larger. Within the science and technology funding area, the authors recommend the following specific allocations, where the larger of the two suggested ceilings should govern. The funding for 6.1 should grow to 1 billion FY 1989 dollars or 3 percent of the RDT&E budget; 6.2 should increase to 3 billion FY 1989 dollars or 7 percent of the RDT&E budget; and 6.3A should grow to 3 billion FY 1989 dollars or 7 percent of the RDT&E budget.

- A <u>new</u> <u>relationship</u> must be made with the defense industry, that will treat suppliers as <u>partners</u> in the <u>planning</u> and <u>thinking</u>, and at <u>arms</u> <u>length</u> in <u>contracting</u>.

- The Office of the Secretary of Defense (OSD) must be strengthened in management, resource allocation and systems procurement. The Joint Chiefs of Staff (JCS) and the Commanders of the Combat Commands must take up more vigorously the role assigned them in the

Reorganization Act of 1986. Together OSD and JCS must work out a clearer vision of military strategy.

- The steps must be taken that are necessary to restore the inventiveness and creativity of the defense technical community. Larger Research and Development (R&D) budgets are needed to restore the vitality we are losing. Better personnel policies assuring pay comparable to the private sector, money for lab equipment, and closer interaction with the military users of future systems are needed. Stronger technical representation in top DoD management councils is required. But most important, generous rewards for excellent performance are required.

- Our way of buying weapon systems must be improved. Weapons platforms last for 30 to 40 years in peacetime. They must be designed from the start to have some of their subsystems, such as guns, radars, and communications, modularized, to make it easy to upgrade these regularly every 5 years or so, should major technical advances become available. Some of this is done now; however, more can be done, and it can be done better. Budget stability will make it easier to plan such upgrades.

In summary, we are facing a great challenge. The tasks are more complex than ever, and the uncertainties are greater than before. Fundamental <u>reform</u> is required, nothing less will do, and this may be the best chance in a lifetime to achieve it.

I. INTRODUCTION

A. MEETING THE TECHNOLOGICAL CHALLENGE

The purpose of this report is to provide an overview of the principal findings and recommendations of the Commission on Integrated Long-Term Strategy's Working Group on Technology. Specifically, subsequent pages outline the Group's views on what is required if the U.S. is to have the technology necessary to implement the Commission's recommendations on how best to ensure U.S. national security into the 21st century. Just as important, however, the authors believe that many of the recommendations set forth in this report must be implemented even if the integrated strategy outlined in Discriminate Deterrence is not fully carried out. Whatever the specific outlines of the U.S. deterrent posture in the future, its credibility to our adversaries and allies alike will depend significantly upon a concerted U.S. effort now to revitalize the defense science and technology base.

Fundamentally, Discriminate Deterrence envisions a future that demands the U.S. plan for a wider range of contingencies than it ever has before. It is not the intent of the Commission's report to diminish the importance of the canonical planning scenarios--a massive Soviet nuclear attack or a concerted invasion of NATO Europe--for U.S. defense policy. Rather, Discriminate Deterrence seeks to bring these traditional defense planning scenarios into balance with a wider range of lesser contingencies that nonetheless may be more plausible in the future security environment. Specifically, the Commission's report points to the need for US defense planning to consider fully the implications of lesser conflicts with the USSR along the Soviet periphery; the advent of potential Third World adversaries armed with highly advanced weaponry; and "low intensity conflict" in the Third World, where insurgencies,

organized terrorism, paramilitary crime, and sabotage threaten U.S. interests.

Since the lesser contingencies have not been at the forefront of U.S. defense planning, no one should be surprised that currently deployed U.S. forces do not always have available equipment embodying technological advances well suited to such conflict environments. Moreover, it is hardly surprising that work in defense R&D is not being guided today in ways that will produce forces able to meet these lesser contingencies in the future. Accordingly, the Working Group initially saw its primary task as twofold: to assay the adequacy of existing technology initiatives and to identify technological opportunities that must be exploited if the U.S. is to achieve an Integrated Long-Term Strategy.

During the course of its efforts, however, the Working Group gradually (and reluctantly) was forced to draw an even more basic conclusion about the adequacy of the current technology. **The current U.S. defense science and technology effort will not ensure the maintenance of a credible defense posture against even the traditional scenarios involving Soviet forces, let alone future contingencies for which little, if any, defense planning has been accomplished.**

B. THE EROSION OF U.S. TECHNOLOGICAL SUPERIORITY

For the past 4 decades, the U.S. has emphasized the qualitative superiority of our forces and those of our allies over the quantitatively superior military forces of our potential adversaries. This emphasis on quality to offset our adversaries' numerical advantages, on balance, has served U.S. national security interests well. Moreover, there is every reason to believe that a continued emphasis on qualitative superiority is

needed for the future. Indeed, it appears to be the only viable course of U.S. action.

However, it remains to be seen whether the U.S. will be able to maintain the requisite technological edge. Having reviewed the evidence, the Working Group is forced to conclude that unless the U.S. makes significant changes in how we realize technological advances and then translate them into weapons systems, this qualitative edge will largely erode by the beginning of the next century.

It is important to look at the <u>trends</u> in qualitative superiority and their impact on military strength, but it is difficult to do. An outline of a possible approach is presented here.

Today, it is clear that the U.S. is superior to the U.S.S.R. in a variety of technology areas. Table 1 provides a comparative overview of the U.S. and Soviet standing in basic technology areas. On a relative basis, the U.S. currently leads in a variety of areas, such as computers and software, electro optics, and guidance and navigation. Further, while the U.S. and U.S.S.R. are judged to be equal in other areas (aerodynamics/fluid dynamics, conventional munitions technologies), in no area is the U.S.S.R. currently judged to be superior.

At the same time, this Table also illustrates the relative trends in U.S. and U.S.S.R. standing in basic technology. Here, the picture is less favorable. While the U.S. likely is widening its lead over the Soviets in computer and software technology, in the remaining areas it is only holding its own or is actually losing ground.

TABLE 1:
RELATIVE U.S./U.S.S.R. STANDING IN THE 20 MOST IMPORTANT TECHNOLOGY AREAS

Basic Technologies	U.S. Superior	U.S./ U.S.S.R. Equal	U.S.S.R. Superior
1. Aerodynamics/Fluid Dynamics		X	
2. Computers and Software	<----X		
3. Conventional Warhead (Including all Chemical Explosives)		X	
4. Directed Energy (Laser)		X	
5. Electro Optical Sensor (Including Infrared)	X---->		
6. Guidance and Navigation	X---->		
7. Life Sciences (Human Factors Genetic Engineering)	X		
8. Materials (Lightweight, High Strength, High Temperature)	X---->		
9. Micro-Electronic Materials and Integrated Circuit Manufacturing	X---->		
10. Nuclear Warhead		X	
11. Optics	X---->		
12. Power Sources (Mobile) (Includes Energy Storage)		X	
13. Production/Manufacturing (Includes Automated Control)	X		
14. Propulsion (Aerospace and Ground Vehicles)	X		
15. Radar Sensor	X---->		
16. Robotics and Machine Intelligence	X		
17. Signal Processing	X		
18. Signature Reduction (Stealth)	X		
19. Submarine Detection	X		
20. Telecommunications (Includes Fiber Optics)	X		

<u>Notes:</u>

1. The list is limited to 20 technologies that in aggregate, were selected with the objective of providing a valid base for comparing overall U.S. and U.S.S.R. basic technology. The list is in alphabetical order. These technologies are "on the shelf" and available for application. The technologies are not intended to compare technology level in currently deployed military systems.
2. The technologies selected have the potential for significantly changing the military capability in the next 10 to 20 years. The technologies are not static; they are improving or have the potential for significant improvement; new technologies may appear on future lists.
3. The arrow denotes that the relative technology level is changing significantly in the direction indicated.
4. The judgments represent consensus within each basic technology area.

Source: Under Secretary of Defense [R&D] Posture Statement to the Congress, Washington, D.C. 20301, 1986.

However, comparing U.S. and U.S.S.R. standings in basic technology provides only part of the picture. It is also necessary to consider the extent to which the two nations translate their mastery of technology into deployed forces. Here, the contemporary picture is less reassuring.

Table 2 compares the level of technology that the U.S. and U.S.S.R. have incorporated in deployed military systems. Despite current U.S. superiority in basic technology, currently fielded U.S. systems are not necessarily technologically superior to their Soviet counterparts. In fact, only in tactical air forces are currently fielded U.S. systems clearly superior across the board in qualitative terms over Soviet systems. In all other broad areas--strategic forces, tactical land forces, naval forces, and command, control, communications, and intelligence (C^3I)--the U.S. is superior in some instances, on a par with Soviet forces in others, and is actually inferior in still others.

Any effort to forecast technology trends and how they are likely to manifest themselves in future force deployments necessarily is speculative. Having reviewed available data, the Working Group concludes that <u>if current trends continue</u>, by the year 2010, the relative standing of the U.S. and U.S.S.R. in terms of system quality, force levels, and overall impact on military capability will approximate that shown in Table 3. This Table reflects the current standing and trends in basic technology and the current and projected ability of both nations to incorporate technology advances in deployed weapons systems. It reflects a US approach that remains what it is now, business as usual. Fundamentally, Table 3 illustrates the Working Group's conclusion that a small and shrinking U.S. technological edge will not offset numerical Soviet superiority to yield equivalent combat capability. In fact, the US may only have a qualitative

TABLE 2: RELATIVE U.S./U.S.S.R. TECHNOLOGY LEVEL IN DEPLOYED MILITARY SYSTEMS

Deployed System	U.S. Superior	U.S./ U.S.S.R. Equal	U.S.S.R. Superior
Strategic			
ICBMs		X	
SSBNs	X		
SLBMs	X		
Bombers	X		
SAMs			X
Ballistic Missile Defense			X
Antisatellite			X
Cruise Missiles		X	
Tactical			
Land Forces			
SAMs (Including Naval)		X	
Tanks		X	
Artillery		X	
Infantry Combat Vehicles		X	
Antitank Guided Missiles		X	
Attack Helicopters	X		
Chemical Warfare			X
Biological Warfare			X
Air Forces			
Fighter/Attack and Interceptor Aircraft	X		
Air-to-Air Missiles	X		
Air-to-Surface Munitions	X		
Airlift Aircraft	X		
Naval Forces			
SSNs	X		
Torpedoes		X	
Sea Based Aircraft	X		
Surface Combatants	X		
Naval Cruise Missiles		X	
Mines			X
C^3I			
Communications		X	
Electronic Countermeasure/ ECCM	X		
Early Warning	X		
Surveillance and Reconnaissance	X		
Training Simulators	X		

Source: Derived from Soviet Military Power, 7th Edition, March 1988

advantage in bombers and cruise missiles. In other areas, U.S. forces will be equal or inferior to their Soviet counterparts.

The outcome anticipated by Table 3 calls into question the ability of the United States--absent fundamental changes in the acquisition process--to offset quantitative disadvantages in the future by qualitative advantages. Fiscal realities alone seemingly would preclude any U.S. effort to match Soviet force levels for the foreseeable future. The conclusion the Working Group draws is that it is mandatory that the U.S. improve its weapon system acquisition process radically, if it is to have a meaningful military posture early in the 21st century.

C. SUBSEQUENT CHAPTERS

The ability of the U.S. to realize a technological edge in the future to offset our adversaries' advantages in numbers is vitally dependent upon redressing serious deficiencies in the U.S. defense acquisition process. Specifically, as outlined in the next chapter, basic changes must be made in the way the U.S. invests in defense science and technology. Further, as discussed in Chapter III, there must also be a major improvement in the philosophy of management that governs technological innovation in military systems.

As necessary as they are, merely redressing deficiencies in investment and management is insufficient. Fundamentally, we need a coherent, long-term technology strategy. The basic outlines of this strategy are sketched in Chapter IV.

The following three chapters present the basic recommendations of the Working Group. Chapter V discusses the technology applications that must be emphasized to implement *Discriminate Deterrence's* recommendations and to redress current deficiencies in the U.S. defense posture. Chapter VI outlines

Table 3:
COMPARISON OF U.S. AND U.S.S.R. FORCES: 2010

	System Quality	Force Levels	Capability *
ICBMs	equal	S	S
SSBNs	US	US	equal/US
Bombers	US	US	US
SAMs	S	S	S
BMD	S	S	S
ASAT	S	S	S
**Cruise Missiles	US	US	US
Tanks	equal	S	S
Artillery	equal	S	S
Helicopters	equal	S	S
Fighter Aircraft	equal	equal	equal?
SSNs	equal	S	S?
Surface Combatants	equal	US	US
C^3	equal	S	S?

*Capability = Overall judgment of the war fighting ability, considering both quality and quantity of the weapons systems
**Land attack cruise missiles only

Legend:

S = Soviets ahead
US = U.S. ahead
Equal = Rough parity between U.S. and Soviets

remedial steps concerning defense investment in science and technology. The final chapter, Chapter VII, sets forth recommendations on overcoming current defense management problems with technological innovation.

II. DOD INVESTMENT IN SCIENCE AND TECHNOLOGY

A. BASIC PROBLEMS

A casual inspection of DoD Research, Development, Testing, and Engineering (RDT&E) spending in recent years easily could lead to the conclusion that investment in science and technology has been more than adequate. After all, DoD RDT&E expenditures have grown in real terms in recent years. (Similarly, combined Government and civilian R&D expenditures have also increased.)

However, the real growth in DoD RDT&E spending masks fundamental problems with how the U.S. has invested in defense science and technology. There are three basic problems with past investment:

- There has been a real decline in front-end investment relative to the early 1960s, the effects of which are probably only now being felt

- Funding for the stage of advanced development most likely to demonstrate the utility of innovative advanced technology applications has been markedly inadequate

- There has been a marked instability in science and technology (S&T) funding levels, which in turn has had adverse implications for innovative advanced technology programs.

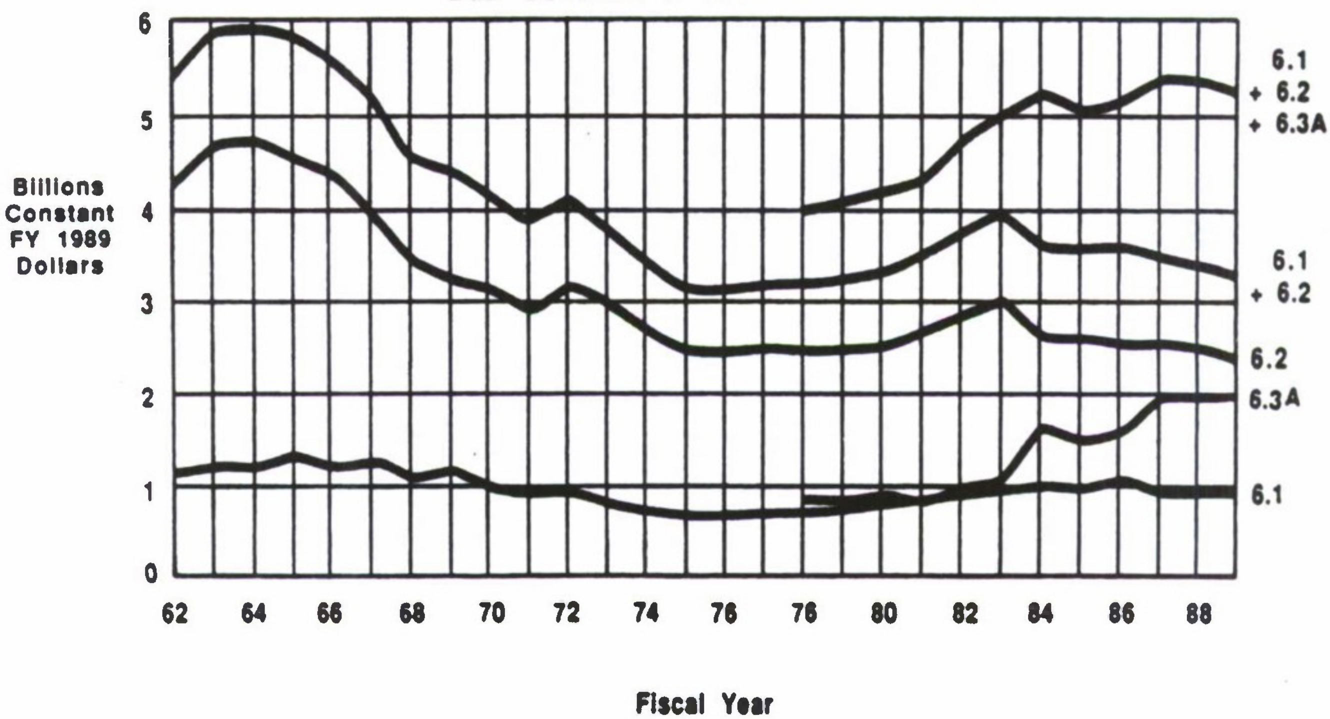
Each of these drawbacks in DoD science and technology investment requires brief consideration. Appendix A provides some of the framework of definitions and description of the R&D process.

B. A RUSTING TECHNOLOGY BASE

Front-end investment in the acquisition process generally includes funding for basic research (labeled Category 6.1), exploratory development (Category 6.2), and advanced development (Category 6.3). Since 1974, Category 6.3 funding has been subdivided into 6.3A and 6.3B. The former subcategory encompasses advanced development work prior to a deployment decision; the latter, advanced development after a deployment decision has been made.

Figure 1 provides an overview--in constant FY 1989 dollars--of 6.1, 6.2, and 6.3A expenditures since 1960. Funds for basic research (Category 6.1), declined sharply in the 1970s from their peak levels in the 1960s, but have remained fairly constant at around $800-$900 million per year since then. (Other sources of funds--from the National Science Foundation and the National Institues of Health, for instance--have led to large increases in

FIGURE 1:
DoD SCIENCE & TECHNOLOGY TREND



DoD Science & Technology Report, March 1988

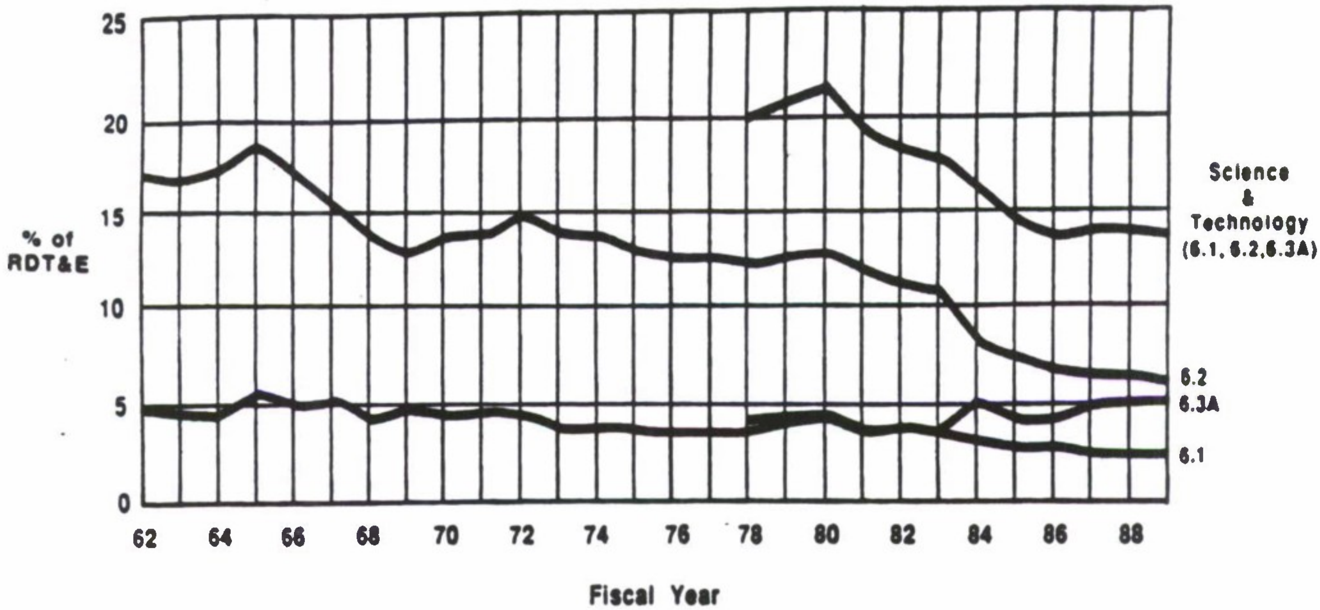
6.3A does not include SDI funding

total basic research, but many of these investments are not related to defense, or only indirectly so.) Funds for exploratory development (Category 6.2) also have suffered from a sharp decrease. This category of research translates basic scientific research (Category 6.1) into concepts with potentially practical applications. In 1964, funding for 6.1 and 6.2 activities combined was at its peak at $5.9 billion (in constant FY 1989 dollars). Today, these activities are currently funded at around $3.4 billion, having slumped in the mid 1970s to a low of $3.1 billion. Similar drops have occurred in the critical 6.3A type of development. The cumulative shortfall in Categories 6.1, 6.2, and 6.3A from the funding peak in the mid 1960s now amounts to roughly $25 billion. This represents a large technical effort: some 250,000 technical man-years that were not carried out, but would have been had the budgets not dropped.

As illustrated in Figure 2, science and technology categories of funding have not grown along with recent increases in RDT&E expenditures. It should be noted this discussion (and the accomanying graphics) of S&T funding levels deliberately excludes recent SDI funding because it's inclusion would present a somewhat misleading picture of the overall DoD science and technology base. As illustrated in Figure 3, substantial funding for Strategic Defense Initiative (SDI) related research was added to the 6.3A portion of the S&T account beginning in FY 1985 (a small amount was added in FY 1984). SDI contributions have amounted to an average of 65 percent of the 6.3A account.

While SDI-related research may have implications for non-SDI development, most of this work is strongly focused on antiballistic missile (ABM) technologies. The true state of the general science and technology base must, however, be viewed with respect to its role in support of broad traditional DoD requirements. Accordingly, for this report, S&T funding is treated without the SDI component of the 6.3A account.

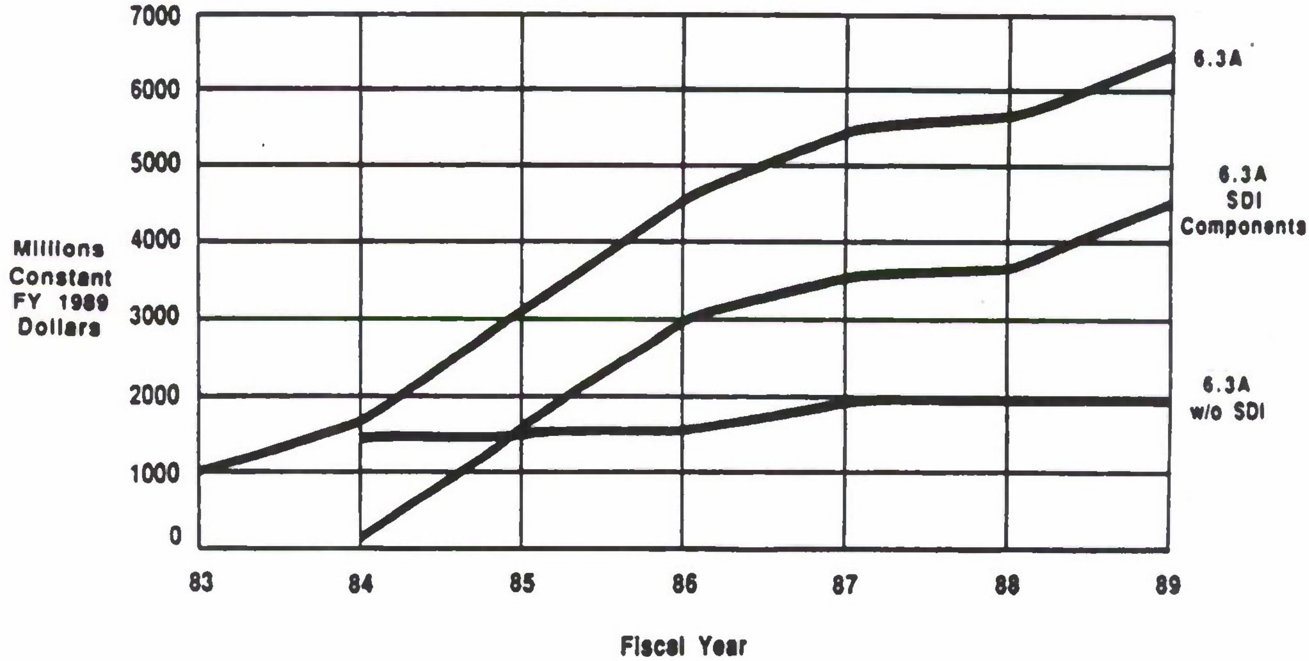
FIGURE 2:
DoD SCIENCE & TECHNOLOGY FUNDING
AS A PERCENT OF TOTAL DoD RDT&E



DoD Science & Technology Report, March 1988

6.3A does not include SDI funding

FIGURE 3:
DOD 6.3A FUNDING



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The cumulative effect of reduced Category 6.1, 6.2, and 6.3A funding--a rusting of the technology base--is only now beginning to be felt. In the R&D and weapon acquisition process (described briefly in Appendix A), it takes roughly 15-20 years or more for basic research advances and at least 10 years for technology advances to be incorporated into deployed weapons systems. In other words, the impact of decreased funding of a decade or more ago is visible in the level of technology in currently deployed systems. Similarly, the effect of today's funding in terms of technology advances will only be obvious in 10 years or so. A continuation of Category 6.1, 6.2, and 6.3A funding at current levels will only serve to perpetuate the process of mortgaging our technological future to realize savings today.

C. INADEQUATE FUNDING TO DEMONSTRATE TECHNOLOGY

The shortfall in funding for Categories 6.1, 6.2, and 6.3A has slowed down our ability to build the science and technology base we need. The second major problem is the level of 6.3A funding itself. This category is particularly important because it is here in the acquisition process that the feasibility and potential utility of technology applications for the solution of militarily significant problems are demonstrated prior to the definition of a full weapon system or a decision to deploy such a system. This is a critical point for technological innovation, and it is at this stage that potential users of a new technology application can begin to weigh its advantages and drawbacks. In short, 6.3A funding represents a gate through which technological innovations in the basic research and exploratory development stages should pass in order to be incorporated into deployed forces.

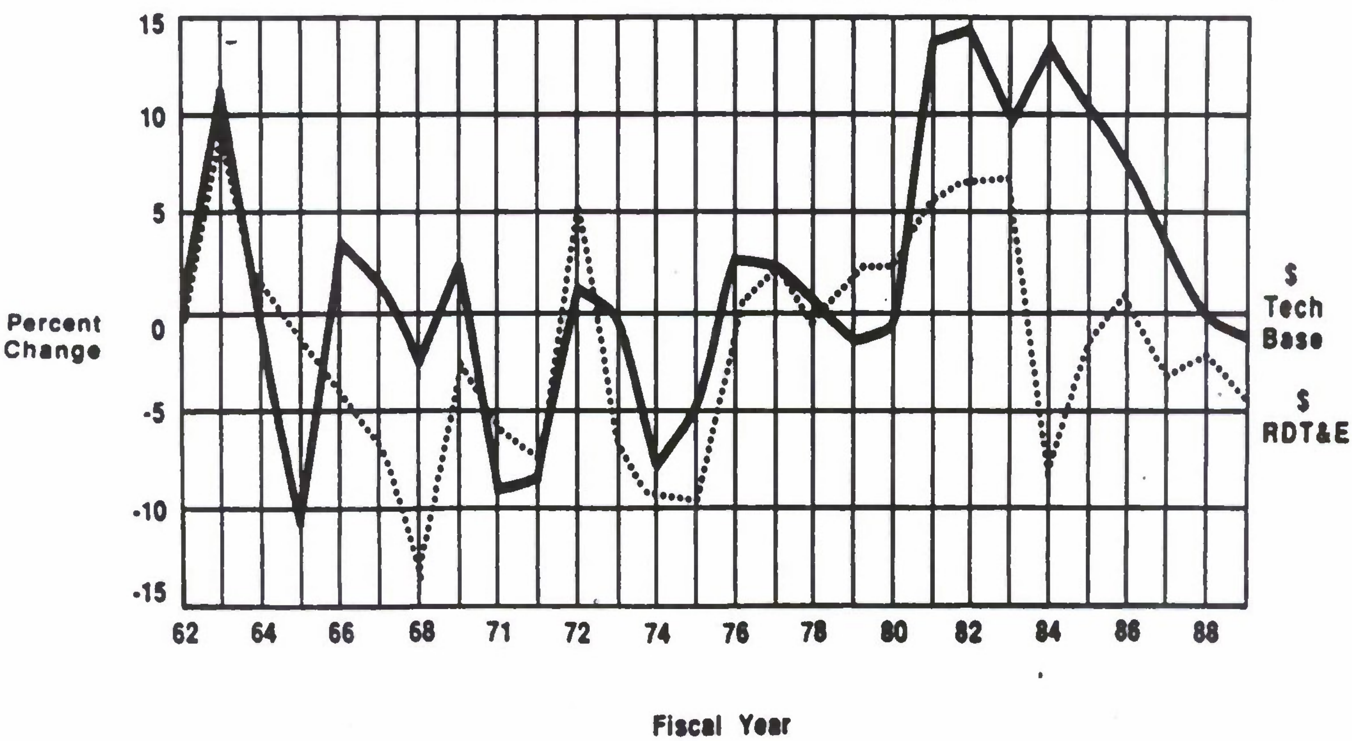
Although, as indicated in Figure 3, 6.3A funding generally has increased over the last decade, it has never been adequate. In fact, it has stayed at between 4 and 5 percent of total RDT&E

funding. For 20 years, this level of 6.3A funding has been a factor in the relatively slow incorporation of some major technological advances into weapons and has precluded the incorporation of others altogether.

D. FUNDING INSTABILITIES AND SHORTFALLS

The third problem is the lack of stable funding for RDT&E in general and for science and technology research in particular. Innovation cannot be accomplished overnight and depends significantly on a stable funding environment over a period of years. As indicated by Figure 4, exactly the opposite has characterized funding for both total RDT&E and the technology base. These fluctuations, of 10 percent per annum or more, are not a recent phenomenon, but rather have been with us for decades. Such fluctuations militate against innovative programs and undermine the quality of R&D efforts, particularly by driving the most creative and talented scientists and engineers out of defense-related work.

FIGURE 4:
CHANGES IN RDT&E AND TECH BASE



The reductions in R&D, aggravated by these instabilities, are a going-out-of-business strategy; no high technology business would survive on the R&D levels we are approaching.

III. A MANAGEMENT PHILOSOPHY THAT IMPEDES INNOVATION

A. RISK AVERSION

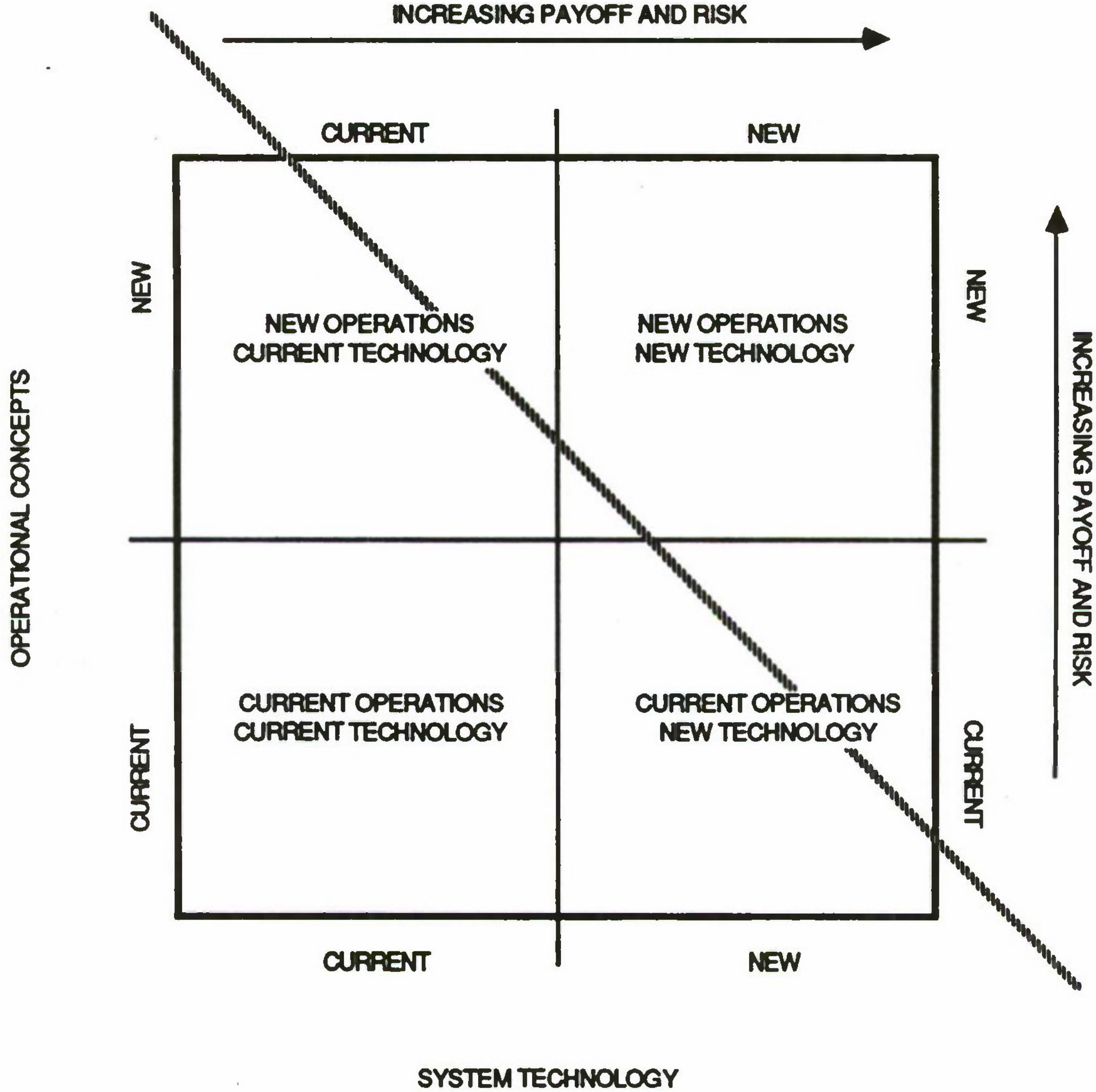
The management philosophy that currently pervades the acquisition process has had the undesired effect of impeding technological innovation, compounding the adverse effects of the investment problems just discussed. Put simply, the current philosophy minimizes any risk of failure of any type. True innovation, by its very nature, comes about after repeated attempts and failures; however, current defense management philosophy increasingly emphasizes an intolerance of any failure. In turn, this risk averse orientation provides a powerful and pervasive disincentive to innovation.

B. A FAILURE TO INTEGRATE INNOVATIVE TECHNOLOGY AND OPERATIONAL CONCEPTS

Second, and relatedly, the management orientation focuses on development programs dedicated to the solution of _current_ operational problems, not the anticipation of _future_ operational problems nor the development of new operational concepts. Clearly, however, significant strides in military capability are often produced by integrating innovative technology applications and innovative operational concepts.

Figure 5 provides a useful conceptual device to characterize the current management philosophy regarding technological innovation and new operational concepts. In Figure 5, operational concepts are divided into "current" and "new" categories, as are systems technology. The majority of R&D activities currently focus on the lower left-hand quadrant, integrating current operational concepts with current systems and technology. This emphasis is ill-suited to the goal of

FIGURE 5:
OPERATIONAL CONCEPTS AND SYSTEMS/TECHNOLOGY



overcoming quantitative shortfalls with qualitative superiority over the long term.

As emerging operational requirements become widely appreciated (or as a result of political imperatives), the Services venture into the lower right-hand quadrant, integrating current operational concepts and new technology applications. To illustrate, the SSN-21 attack submarine is essentially predicated on existing operational concepts, but incorporates new systems and technology at relatively low risk.

The upper left-hand quadrant represents potential new operational concepts support by current systems/technology. An example is AEGIS: the technology and systems that have gone into AEGIS have been available for the last 15 years, but the Navy is only now able to exploit the new operational capabilities provided by AEGIS and its systems.

The upper right-hand quadrant focuses on new operational concepts utilizing new systems and technology, in other words, highly innovative technology applied in highly innovative ways to achieve dramatic increases in military capability. Long-range bombers, nuclear submarines, and solid propellant missiles are historic examples. Stealth in its several applications is an important example. Clearly, this is an approach that the U.S. should emphasize if it seeks to overcome numerical shortfalls by a qualitative superiority. Also, the competitive strategies envisioned by the Secretary of Defense must venture into the upper-right quadrant.

However, too little DoD activity concentrates on the opportunities that could be realized by integrating new operational concepts and technological innovation. This is the area where the technological gains offer the highest military payoff, but there is also the higher risk of some failures. The

current DoD emphasis on competitive strategies is a useful step in this direction.

Nonetheless, most DoD development organizations tend to avoid risk and, therefore, their RTD&E activities concentrate on conservative projects that are the most likely to succeed in the sense of meetin cost, schedle, and performance specifications. They tend to operate in the area below the dotted diagonal line in Figure 5. DoD in fact develops quite well those operational concepts and systems/technologies associated with the bottom half of the figure. It is clear why this is so. The decision-making processes that determine the development activities are: (1) driven by formal requirements and largely supported by in-house RDT&E centered activities; (2) based on planned product improvement, which exploits evolutionary rather than revolutionary concepts and systems/technologies; and (3) excessively devoted to risk avoidance. While most militarily significant problems can and should be addressed using this approach, we will <u>not</u> be able to exploit to the fullest our ability to develop and field truly new advanced technology solutions, if this conservative approach is presented as the yard stick against which <u>all</u> development programs are measured.

In contrast to the above, the institutions and processes available in the DoD are inadequate to couple innovative technology with new operational concepts. Such enterprises generally have the characteristics of the classic "skunk works": (1) success cannot be predicted with certainty; (2) the effort is not driven by formal requirements, but by perceived opportunities for novel operational capabilities; (3) the process is not part of planned product improvement; and (4) the process may lead to revolutionary operational capabilities that cannot necessarily be envisioned at the outset.

Dramatic increases in future combat superiority are likely to come from the integration of new operational concepts with new systems and technologies. This in turn can only be realized by a concerted effort to manage high degrees of risk and a tolerance for the inevitable failures occasionally associated with innovation.

C. ADVERSARIAL CLIMATE OF TECHNOLOGY DEVELOPMENT

Finally, the current relationship between the DoD, its industrial contractors, and the Congress is best characterized as one of distrust, full of suspicion and frustration, yielding ever increasing legal requirements, which results in the growth of a bloated bureaucracy attempting to oversee, without responsibility for results, every detail of every phase of the development and acquisition process. This environment places the program manager in an untenable position. The program manager must answer to numerous oversight agencies, committees, and organizations, each of which feels it has the final decision in the program's outcome. Moreover, in practice, any one of these oversight organizations has the ability to block or divert an ongoing program, whereas none of them can ensure a program's continuation. This results in a stop-go, go-somewhere-else, random motion of programs, which has demonstrably cost much in time, money, and losses of talent at all levels.

Advanced, innovative technology simply cannot be developed or applied in this environment. Program delays, cost overruns, and even potential failures are all part of developing high-risk but high-payoff advanced technology systems. While some oversight is essential, it must not become the focus and end of the management process. Managers of high-risk programs must be rewarded for their work, and program uncertainties must be accepted by all as the nature of high-risk, high-payoff advanced technology development programs.

IV. A TECHNOLOGY STRATEGY FOR NATIONAL SECURITY

A. THE NEED FOR A TECHNOLOGY STRATEGY

The basic problems that currently beset the application of defense technology and innovation in the U.S. acquisition process stem from the absence of any explicit defense technology strategy. Today, there is no coherent, long-term strategy that sets basic objectives and investment and management approaches for technology development and prescribes the suitable introduction of technology into the acquisition process. Instead, to the extent that there is any "strategy" at all, it is one that has emerged in an ad hoc fashion as the result of funding, management, and other decisions that have been made on a case-by-case basis. If strategy is best indicated by actions, ours is a going-out-of-business strategy, not a qualitative leadership strategy.

In this report, "strategy" is used to mean a concept that links ends and means--or objectives--with resources and actions. As such, it must spell out where we want to go, what resources we have available for use (or what we can generate) to get us there, and what steps or actions we must take to reach our objective.

The Working Group on Technology believes that there is an urgent need for the establishment of a national technology strategy that clearly sets objectives, outlines principles for the strategy's implementation, and identifies specific means to attain the objectives. Given the authors' work, that of others under the Commission's charter, and the results of recent, related groups (such as the Packard Commission and the Defense Science Board), this Working Group believes that a national technology strategy should set the objectives and embody the principles that follow.

B. OBJECTIVES

The first objective should be to continue emphasizing technologically superior U.S. forces. This objective has served U.S. interests well in the past and is even more relevant to a future likely to be characterized by increasing resource constraints. Without a doubt, however, this objective is now far more difficult to attain than it ever has been and is only likely to become increasingly more difficult to realize in the future. The reason for this is simple: the Soviets are becoming increasingly more adept at incorporating technological innovation into their own forces. Indeed, their mastery over military technology has made significant strides during the very time they have been experiencing acute domestic economic difficulties.

Second, the U.S. should exploit technological applications appropriate to the future security environment. This means emphasizing technologies that help us prepare for the lesser and perhaps more likely contingencies--limited wars along the Soviet periphery, conflicts with well-armed third parties, and low intensity conflict--as well as large-scale conflict involving the forces of the Soviet Union and its allies. This objective implies emphasizing the emerging technologies of precision, control, and intelligence and the technologies that make discriminate weapons possible. It also suggests exploring technological avenues that will assist U.S. forces operating in remote and hostile parts of the world, perhaps without the benefit of foreign bases, overflight rights, and other host-nation support.

Third, we should strive not merely to introduce new technology applications that fit existing operational concepts, but also to search for combinations of new technology applications and new operational concepts. There can be little doubt that technology will help our military forces to execute

their existing operational concepts with correspondingly beneficial implications for our defense policy. The most dramatic strides in operational capabilities, however, will only come about via innovation, not just in technology, but in the way the military employs new weapons. Thus, we must emphasize the novel combinations of new technology with new operational concepts to the fullest extent possible.

C. CONCEPTS

Four basic concepts must guide our actions if we are to attain the major objectives of our national technology strategy.

First, the vigorous production of technology, and by implication, the maintenance of a strong defense industrial technology base is an essential, irreplaceable national asset. At first glance, this appears so obvious as not to merit mention. However, it is easy to lose sight of this fact as financial and other resources become scarce or as immediate defense problems capture our attention and overwhelm available resources. Explicit recognition of this principle implies a long-term vision of the future and a commitment not to mortgage that future to realize small savings today or to resolve transient problems.

Second, as a consequence of the above, U.S. science and technology investment patterns must be consistent with our long-term objectives. In turn, front-end investment--Categories 6.1, 6.2, and 6.3A--funding must be increased to a viable level. Additionally, long-term stability must be imparted to the science and technology base funding process. We can ill afford the fluctuations on an annual basis that have characterized past investment patterns in defense science and technology. And the major efforts must be focused on fields where scientific breakthroughs are happening and where these breakthroughs open

the possibility of qualitative improvement in mission performance.

The message that science and technology funding must be not only increased, but also stable for the long term may appear unrealistic in the current U.S. fiscal climate. The Working Group believes, however, that any future savings in defense spending must not be achieved at the cost of the defense science and technology effort. A long-term view that envisions the probability of continued competition with the U.S.S.R. or other major powers suggests investment today in science and technology to be properly postured tomorrow should new security challenges arise.

The third concept is that defense management must emphasize again the integration of a broad range of talents in the science and technology efforts. We must make better use of the talents that reside in the universities than we have recently. Further, it is essential to strengthen the partnership between Government and industry in the R&D process, without compromising in any way our commitment to the highest legal standards in defense contracting. There is also a need for the operational commands to become more involved in the early decision phases of the acquisition process. Innovative operational concepts often are developed best if military staffs are involved early with technology concepts.

Finally, the philosophy of innovation must be injected again in the entire acquisition process. Participants in the acquisition process must be made aware that a premium is being placed on innovative results. Failures arising from ambitious goals are to be not only tolerated in research and exploratory development, but also expected, because repeated tries are a

natural part of innovation.[1] The reward structures in the R&D and system acquisition processes need to be consonant with the premium placed on innovative results. To attract the best talent, provisions must be made for appropriate incentives, both for the individuals and organizations that participate in the process.

D. MEANS

The next three chapters outline the specific means the Working Group on Technology recommends to achieve the objectives of the national technology strategy.

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[1] These remarks, and others like them in this report, in no way condone or encourage failures in programs arising from incompetent performance, lack of commitment, poor management, and other similar causes. The failures that must be allowed are those that arise from trying for huge results, perhaps a little too soon, based on excellent talent, reasonable resources, and daring objectives.

V. THE EFFECTIVE USE OF TECHNOLOGY

Technological factors are an essential contributor to a nation's military capability. This contribution is not limited to the development of weapons and weapon systems, but provides significant insights for developing and supporting novel operational concepts. Throughout history, technology, as applied to the military arts, has been instrumental in shaping the role of nations. The introduction of tempered steel in sword-making a thousand years ago gave the Arab nations military superiority for centuries. The introduction of the longbow was a significant cause of the British victories over the French at Crecy and Agincourt in the Middle Ages. More recent examples, such as the introduction of machine guns and barbed wire, abound.

A. THE CRITICAL IMPORTANCE OF THE BASIC SCIENCES

Fundamental to the progress of technology is the advancement of research in the basic sciences. To get some idea of what may be possible in technology 20 years from now, it is necessary to look at what is happening in the laboratories today. The review of the progress in basic science research must be ongoing and an integral part of the long-range planning process.

While continuing research in the basic sciences can be expected to yield technology applications as a matter of course, discontinuities do occur; genuine surprises happen. They do not happen often, and it is impossible to predict when such surprises may occur. In many instances, this type of breakthrough, if recognized quickly and properly developed, can lead to major new technology capabilities. The Nation's scientific and technical establishment must maintain an open mind toward and interest in basic research in order to take advantage of these breakthroughs when they do occur.

In spite of the problems outlined in earlier chapters, the United States still has the world's best scientific research establishment. Much of the basic scientific research done in the United States is conducted in the universities. American universities continue to provide world leadership by maintaining high standards of quality both in education and scientific research. There are many measures that demonstrate this leadership: the United States still wins more Nobel Prizes than other nations, and there is little doubt that American scientists and engineers are at the cutting edge of scientific research. Another measure of the quality of American universities is the number of foreign students that come to this country to complete their higher education.

B. BASIC RESEARCH IN UNIVERSITIES GENERALLY WELL FUNDED

Basic research in American universities, in general, is well funded. The Federal Government provides between $5 and $6 billion a year to support university-based research. Approximately another $1 billion comes from other sources, including private and industrial contributions and state and local political entities. Faculty members at American universities are given remarkable freedom to determine the areas of inquiry where this investment is spent. The system of Federal funding for university research is sufficiently loosely coupled to practical applications to allow investigators the freedom to do what they believe to be important and intrinsically interesting in their own scientific disciplines. This is a uniquely American approach, based on the idea that genuine progress in the advancement of human knowledge can only be made within an atmosphere of academic freedom. Some minor structural problems in the funding mechanism (for example, the difficulty of getting small research grants in some fields) are real and should be corrected, but need not be addressed here.

In order to provide a framework for further assessment of what might be possible in the area of militarily applicable technology for the next decade or two, five areas of major science disciplines are highlighted below. The focus is principally on the physical sciences and their probable application. It is very likely, however, that the biological and medical sciences will bring advances to technology (broadly speaking) that will be just as important in the long run as those derived from the physical sciences. However, these biology-derived applications are harder to predict at this time, and therefore the authors stress here what can be described with greater confidence. The five major areas are:

- The physics and chemistry of matter. This area includes quantum mechanics, solid state physics, particle physics, thermodynamics, statistical mechanics, chemical reactions, and biological physics and chemistry.

- The theory of radiation and its interaction with matter. This includes electro-magnetic theory based on the Maxwell equations, the theory of relativity, quantum electro-dynamics, and related areas. Cosmology and the physics of the gravitational field are also part of this area.

- Transport physics. This discipline includes the transport of matter, radiation, and energy in very general terms. More specifically, fluid flow, heat transfer, and radiative energy absorption and reflection are included. These are generally based on the Boltzmann equation and its various approximations as well as the laws of physical optics.

- Mathematical physics. This is perhaps the most basic area, which includes the application of new mathematics to computational physics, statistics, the theory of knowledge, and related areas.

- Biosciences. This covers the physics and the chemistry of living matter at levels of organization ranging from the molecular through large-scale plant and animal life all the way to the understanding of the ecosystems composing the biosphere. The scientific disciplines include the modeling of molecules and their interactions in the reproduction and growth of living cells; the chemistry of enzymes inhibiting or accelerating biological reactions; and the molecular basis for genetics and the associated investigations of congenital defects, the anatomy, physiology, and disfunctions of the human body as related to the newly acquired fundamental concepts in biology. The relevance of bioscience research to the problems potentially facing the Department of Defense include: (1) medical advances in the prevention of epidemics in the armed forces; (2) health services to active and reserve military personnel; (3) emergency services for combat-related wounds and other disabilities; (4) the effects of ionizing radiations on humans and other organisms; (5) detection and protective mechanisms against chemical and biological agents used in overt or covert modes of combat; (6) evaluation of the threat posed by the possible use for aggressive purposes of chemical and biological agents by potential adversaries; and (7) sanitization, decontamination, and preservation of consumables for the armed services.

There are deep relationships between these areas and some may overlap. The definitions are somewhat arbitrary and the

lines might be drawn differently. Details on these basic science areas can be found in Appendix B.

Understanding the state of the basic sciences, as discussed in Appendix B, will provide insight into what can be expected to emerge as technologies in the coming decades. Technologies that are currently under development must be explored as the source of solutions to many of the problems identified in Discriminate Deterrence. Current and developing technologies will provide many significant capabilities. These technology areas are discussed in the following section.

C. THE EFFECTIVE USE OF TECHNOLOGY FOR THE FIELDING OF MILITARY SYSTEMS

The implementation of the Discriminate Deterrence strategy is heavily dependent on the growth of technology. The requirements to respond to a wide range of contingencies; to develop versatile, highly mobile forces able to deliver precisely controlled strikes at great distances; to develop both a strategic defense and a deep, counter-offensive operation capability as well as to address the ability to control space during wartime are all heavily dependent on technology. It is essential to the Discriminate Deterrence strategy that a healthy and aggressive capability to develop technologies and their implementation as military systems be maintained.

Underlying all the technologies essential for implementing The Commission's strategy are three key technologies that enable the advances of the future. They are computer technology (architecture, hardware, and software), materials technology (structural and electronic materials), and sensor technology (sensor systems, components, and information processing). All three of these key technologies depend on each other, and this mutual dependence and support has fueled explosive growth in the

capabilities made available by these technologies. In turn, they depend on the basic physical sciences listed above and described in Appendix B.

The Working Group's review of technology areas focused upon those which would contribute to the successful implementation of Discriminate Deterrence and to redressing existing deficiencies in the U.S. force posture. Specifically, the following criteria were used:

- Technology areas needed to support the long-term strategy of Discriminate Deterrence
- Technology areas that, if implemented and deployed, would exert the highest leverage in solving some critical military problems.
- Technology areas given a low priority or opposed because their potential contribution to the solution of important military problems has not been fully understood.

Applying these criteria resulted in the identification of the following technology areas:

- Training aided by computer simulation
- Stealth (low observables)
- Small satellites (C^3I)
- Accurate long-range cruise missiles
- Ballistic missile defenses
- Nuclear, earth penetrating weapons
- Advanced non-nuclear munitions
- Airplanes, flight vehicles
- Surface effect ships
- Submarine technology and antisubmarine warfare.

Subsequent paragraphs describe briefly each technology area's potential contribution to the strategy of Discriminate Deterrence and identify any problems now being encountered in the development of the technology.

1. Training Based on Computer Simulation Modern techniques for computer simulation of military systems coupled with communications technology can provide the capability for connecting together hundreds or thousands of training simulators. This capability, if properly exploited, would provide for realistic simulation of complex, multilayered military operations. As new, complex and costly systems are fielded, the opportunity for sufficient live training exercises may be limited. This type of realistic simulation can provide for effective training when used in conjunction with field and live fire exercises, enhancing the overall combat effectiveness and readiness of our military units. An excellent example of such a program is SIMNET, a joint product of the Army and Defense Advanced Research Projects Agency (DARPA). Similar systems could be designed for troops and commanders at all levels of responsibility.

Some training of this type is currently being done, however expansion of these efforts currently is not a priority item. This lack of priority perhaps arises because the high leverage inherent in simulated training is not widely enough understood. Technology for training simulations is currently available requiring only the integration and development of specific training systems.

2. Stealth (Low Observables) As Discriminate Deterrence points out, low-observables technology is revolutionary. Radar systems that locate, track, and attack traditional military aircraft and vehicles are relatively inexpensive and quite effective. Replacing these radar systems with systems that locate, track,

and attack stealthy vehicles will be expensive and technically difficult. The full impact and capability of stealth technology has yet to be determined and a major effort is required to make this technology fully effective.

The importance and utility of Stealth technology can best be understood by looking at an historic example. One of the earliest examples of the effectiveness of a low-observables capability was the World War II Mosquito bomber of the Royal Air Force. While this all-wood frame bomber is perhaps best known for its ability to hit targets with pinpoint accuracy (its breaching of the walls at Amiens prison to release Gestapo captives is legendary), it was the Mosquito's ability to fly undetected because of its wooden airframe and its speed at very low levels that gave it one of the lowest loss rates and greatest reputation for effectiveness among World War II aircraft. For example, during their daylight attack on Gestapo headquarters in the middle of Copenhagen, the planes arrived so stealthily and so suddenly that the covers were never removed from the defending guns. Perhaps the overwhelming importance of low-observables technology is that it may make tactical surprise feasible again.

The details of this technology are sensitive subjects and cannot be discussed in this report. The authors have serious concern that the isolation resulting from these security arrangements makes it very difficult to treat low-observables technologies in a system context. This isolation makes it difficult to develop coherent, integrated operational concepts with current technology as well as develop new operational concepts to provide technology development with future direction. The effect of this technology on munitions, support infrastructures, and the potential consequences of effective countermeasures cannot be fully addressed here, but the Working Group believes that these issues are not being given the attention they require. See Appendix C for additional views.

3. <u>Small Satellites</u> Satellites can be used to acquire and distribute information about an adversary's readiness status, force location, and movements around the world. These systems also perform vital functions in the coordination and communications required for our military operations worldwide. These capabilities provide large incentives for an aggressor to attack and defeat our satellite capability. Current U.S. satellites and ground support systems are vulnerable. Most were designed primarily to execute exacting peacetime tasks or provide warning of the outbreak of a war, and are located in a few well known locations. These satellites tend to be large and expensive, requiring large launch vehicles utilizing a small number of vulnerable launch facilities.

The Commission report recommends the gradual introduction of advanced technology satellites with useful payloads that are light enough to be launched by small, mobile launch vehicles. This would provide the United States with a robust, reconstitutable military space component. Important progress has been made in developing the technology for such systems, but the management problems remain formidable.

4. <u>Accurate, Long-Range Cruise Missiles</u> A key element of the <u>Discriminate Deterrence</u> strategy is the ability to deliver precisely controlled strikes deep into enemy territory. Cruise missiles with long range (thousands of miles) and very high accuracy (approximately 1 meter Circular Error Probable (CEP)) will be required to support this element of the overall strategy. These weapons should be dual-capable, delivering either nuclear or non-nuclear warheads.

Even though current intelligence assets can provide the U.S. with accurate information about the placement of many critical military targets, our current missile systems cannot defeat these targets without inflicting excessive, and perhaps intolerable,

levels of collateral damage. As missile system accuracy improves, the warhead size required to defeat these strategic targets drops dramatically. This results in fewer total weapons required and holds collateral damage to very low levels or possibly, in some instances, avoids it entirely. Progress is being made toward achieving such system accuracies; however, their, incorporation into fielded systems has been delayed because their utility has not been adequately appreciated.

Several types of these systems are needed, with ranges from as low as tens of miles to ranges of several thousands of miles. Naturally, the costs of these different systems would vary widely, and many more of the shorter range (and cheaper) missiles needed than those with the longer ranges (and higher costs). See Appendices D and E for additional information.

5. <u>Ballistic Missile Defenses</u> The Soviet Union is improving its active missile defenses, including the deployed radar and C^3 infrastructure, and will likely be in a position to extend its missile defense capabilities with relatively short lead times. At the same time, the Soviets are eager to deny the U.S. an equivalent capability. These two facts, quite separate from arguments about the merits of the Strategic Defense Initiative or the interpretation of the ABM Treaty, highlight the necessity of research and development on some active defense capability against ballistic missiles. The potential leverage of this technology will certainly spur development in other countries in the coming decades.

Ballistic missile defenses of even modest capabilities can contribute to deterring Soviet attacks on many different targets. The Soviets would not be able to predict easily which targets were defended and, consequently, would not be able to predict which missiles would reach their targets. Soviet attack planners would become less certain of success, drastically increasing the

number of weapons Soviet war planners would require to restore confidence in their plans and gain high assurance of success in execution.

For this reason, the Working Group supports the early development and deployment of a modest antiballistic missile capability to defend national and other C^3I facilities as well as some other high-value military targets. This level of missile defense appears to be technically feasible and affordable.

6. Nuclear, Earth-Penetrating Weapons The Soviet leadership is increasingly protected by passive means in addition to active missile defense systems. It is imperative that the United States be able to hold key parts of this command structure at risk. Soviet construction of underground facilities, hundreds of meters deep, has created a difficult targeting problem. Even "near zero" CEP systems would require several weapons or a large yield to defeat only one deep facility whose location is known exactly. If location of underground facilities is somewhat uncertain, the situation is much worse. Earth- penetrating weapons employ greatly enhanced ground shock as the kill mechanism. This targeting method requires both fewer weapons and lower yields to hold Soviet command and control at risk. A side "benefit" of reduced collateral damage would also result from the use of earth-penetrating weapons.

Earth-penetrating weapons support the long-term strategy of Discriminate Deterrence and exert high leverage on a critical military problem. The capability exists today to merge current technology with new operations in order to discriminately hold a valuable target set at risk. A superb example of exploiting technology without the normal 10 year delay (from exploratory development to deployed system) is the work on earth-penetrating weapons. The concept of modifying existing weapons and incorporating them into existing systems, which then results in

an earth-penetrating capability is exemplary. Earth-penetrating warheads should be developed and deployed.

7. Advanced, Non-Nuclear Munitions Advanced, smart munitions technology is perhaps the technology with the largest potential leverage on the combat effectiveness of U.S. forces. The potential to destroy armored targets with artillery and missiles and to successfully engage protected fixed targets with a conventional capability could have a major impact on our ability to deter aggression worldwide within the framework of the Discriminate Deterrence strategy. The systems applications range from rocket and artillery shells, standoff missiles, and air-to-air and air-to-ground ordnance to torpedoes, mines and many others.

Presently, the available and emerging technologies are being applied in a piecemeal and ad hoc manner. Technology with this broad a range of applications and with this large, potential impact on our force capability requires a more coordinated effort.

8. Airplanes, Flight Vehicles Significant research into aviation technology is currently being performed. Advances in materials technology, aerodynamics, energy systems applied to propulsion, and electronics--all point toward significant technology capability being available to support the Discriminate Deterrence strategy.

Lightweight materials are always a requirement in aircraft construction, and advances in composite materials will continue to meet these requirements. There are, however more exotic materials applications currently being explored. The use of materials with anisotropic properties for increased strength and tailored materials with unusual electronic and electrical properties supporting stealth aircraft programs are examples.

The use of supercomputers to calculate complex flow fields with great accuracy has led to a genuine breakthrough in aerodynamics technology during the past decade. Very complex aerodynamic shapes can now be considered for incorporation into future aircraft programs. Supercomputers have truly become the numerical windtunnels that were imagined in the early 1970s.

Electronics technology has been advancing at astounding rates with applications across many technology areas. Aviation is no exception. The growth in sensor technology, coupled with ever increasingly powerful small computers, promises to yield significant combat capability for future aircraft programs. The entire field of avionics and aircraft control systems will be transformed by these advances in electronics technology.

9. Surface Effects Ships Surface effects ships can develop speeds up to twice that of conventional hull vessels. This technology can provide a significant rapid sealift capability. While surface effects ships do have range and payload limitations, they could be used to enhance greatly our rapid-response capability.

Discriminate Deterrence and the report of Offense-Defense Working Group point out that it is likely crises will arise for which we may wish to introduce rapidly and over large distances relatively small forces when Marine Amphibious Forces may not be available and where it would be difficult to use air transport. A few large surface effects ships would provide for such a quick-reaction contingency. This proposal is usually associated by most critics with efforts to resupply Europe during a major war. For such a requirement, this type of vessel would be inefficient. However, conflicts in regions remote from the United States and Europe are becoming increasingly likely, and the ability to insert troops in 3 days instead of 6, would provide a significant

capability to react to these contingencies. Small surface effects ships are now being introduced into the Marine Amphibious Forces, where they will play an important role for short-range lift applications. The authors propose selective introduction of a few large Surface Effects Ships (SES) for long-range force application.

10. <u>Submarine Technology and Antisubmarine Warfare</u> The trends forecast in <u>Discriminate Deterrence</u> clearly point to the increased relative importance of submarine warfare. The Soviet Union has invested impressive resources in the increase of functional performance of its submarines, as well as in the reduction or control of acoustic signatures; it is also well known that their Antisubmarine warfare (ASW) technology progress is no less impressive. It can no longer be assumed that the U.S. will be dominant in future undersea warfare engagements, whether strategic or tactical. Our potential adversary could well match us in number and type of platforms, in the characteristics of weaponry, in the use of countermeasures, and in the command and control of naval forces, including submarine, surface, and airborne units and, most significant, the effective integration of space-based assets.

Central to effective development of U.S. technology will be better integration of all the underwater detection and classification techniques. Further, it will be necessary to invest in several advanced submarine platforms and in the effective coordination of several platform types under actual combat conditions. Submarine-borne, long-range conventionally armed or nuclear weaponry, together with the targeting and kill assessment functions, should receive renewed attention.

VI. INVESTING FOR THE FUTURE

A. FRONT-END INVESTMENT RECOMMENDATIONS

Long-term investment in our science and technology base (the Category 6.1, 6.2, and 6.3A accounts) is essential. The realization of defense capabilities that maintain a significant qualitative edge into the next century can only be achieved through a focused science and technology investment effort. This effort must begin now. To assist in focusing our investment for the future, the Working Group recommends that the U.S.:

- Maintain a steady commitment to the science and technology base through a long-term, stable funding strategy

- Stop the rusting of our technology base; science and technology accounts should grow at a slightly higher rate than total RDT&E

- Widen the 6.3A gate, allowing accelerated incorporation of new technology into deployed capability

- Ensure that fixed-priced R&D contract procedures and excessively obstructive competitive procedures are removed as an obstacle to the development of advanced technology and are not re-introduced.

B. A COMMITMENT TO STABLE FUNDING

The funding of DoD science and technology must not be reduced; it must be increased, if necessary, at the expense of force structure. The <u>Discriminate Deterrence</u> strategy clearly illustrates a future requiring the development of advanced technology, coupled with innovative operational concepts, to meet

a much wider range of contingencies than in the past. This future can only be realized by maintaining a strong science and technology base. Both the Congress and the Executive Branch must maintain a long-term commitment to this effort. Funding stability is critical, not only for the direct level of effort it affords, but also to attract the high quality of people essential to success.

C. INCREASE SCIENCE AND TECHNOLOGY FUNDING

To stop the rusting of our science and technology base, science and technology funding must grow at a slightly faster rate than the rate of total RDT&E funding.[2] This accelerated growth rate need only be 2 to 3 percent above the rate of total RDT&E growth. Should total RDT&E remain constant or even decline, the science and technology accounts should still grow (at a rate around 5 percent). In either event, we would envision S&T funding growing until it totals 17 percent of RDT&E or 7 billion FY 1989 dollars, whichever is larger. The U.S. is entering a period of explosive technology growth, especially in the areas highlighted in Chapter V and Appendix B that are essential to many future defense applications. Only through a commitment to a strategy of gradual long-term growth in the science and technology base will we be able to reap the fruits of this technology growth in the future decades.

Within the science and technology funding area, the Working Group recommends the following allocations, where the larger of the two suggested ceilings should govern. The funding for Category 6.1 should grow to 1 billion FY 1989 dollars or 3 percent of the RDT&E budget, and Category 6.2 should increase to 3 billion FY 1989 dollars or 7 percent of the RDT&E budget.

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[2] The key investments in R&D that would assure stable and aggressive programs are not large compared to the procurement account, and are much more highly leveraged than the latter.

The Category 6.3A account also requires a substantial increase. Specifically, 6.3A funding should grow to 3 billion FY 1989 dollars or 7 percent of the RDT&E budget. This funding represents the gate through which technological innovations reach demonstration of feasibility for eventual deployed capability. As discussed in Chapter II, this area has, over the past decades, been underfunded. This chronic 6.3A underfunding is in part to blame for the propensity to incorporate only excessively mature technology in the fielding of our defense systems. Current 6.3A funding levels afford very limited technology alternatives to program managers. This limitation results in the risk-averse approach of selecting only mature technology very early in the procurement cycle.

Technology mature enough to make advanced system development possible is not necessarily a low-risk technology. High-risk technology must also be considered during the advanced development process. To explore fully the potential of advanced technology, 6.3A funding must be maintained at levels providing for possible failures as well as successes. We recommend, for planning purposes, that within the S&T accounts, 6.3A accounts maintain a growth rate twice the S&T rate, until it is approximately 10 percent of total RDT&E funding. This funding strategy will provide the defense community the ability to explore, in the advanced technology arena, several technology alternatives. Only by providing for this capability will we be assured that we are applying our best technology efforts to support new innovative operational concepts.

D. ABOLISH FIXED-PRICE R&D CONTRACTS

In research and development, excellence has a disproportionately large impact. While fixed-price R&D contracts *per se* are no longer used by the DoD, technology development programs bid and won on a labor rate structure are in essence

fixed-price R&D contracts. This procedure is a major obstacle to the excellence required for the development of advanced technology. R&D contracts cannot be viewed as comparable to contracts to purchase commodities; competition in contracting, while desirable, has limited applicability to basic research and exploratory development if innovation is to be achieved. The award of an R&D contract based on a total program cost estimate made well in advance of the actual R&D effort can only ensure that technological risk is minimized throughout the life of the program. Advanced technology development must be accomplished in a risk-taking environment. Programs must be funded to provide for the inevitable uncertainties characteristic of emerging advanced technology development. Program delays and cost overruns must not be anathma in advance technology development and funding should recognize that they will sometimes occur if DoD expects to realize the benefits of advanced technology development.

E. SUMMARY

Science and technology investment is critical to supporting the goal of developing advanced technology in support of new operational concepts. This investment must be viewed from the long term. Technology development begins with research in the basic sciences, develops in the laboratories of both Government-sponsored agencies and industry, and finally emerges as a candidate to support operational concepts critical to our national security. Funding shortfalls, due to short-term fiscal decisions, will have devastating long-term effects on this process. The Working Group on Technology strongly urges that both the congress and the Department of Defense make a commitment to long-term investment in science and technology.

VII. MANAGING FOR THE FUTURE

A. OVERVIEW

We believe that many opportunities in science and technology will be available to the Department of Defense in the coming decades, particularly if this report's recommendations for investment in the future are implemented. Identifying these opportunities, developing them, and integrating this advanced technology into useful defense capability will require that the management of the defense acquisition process undergo major changes. Many of these changes have already been identified in the June 1986 National Security Planning & Budgeting report of the Packard Commission. The Working Group on Technology fully and emphatically endorses these recommendations but believes it is necessary to build upon and go beyond them.

Successful management of technology development within the defense acquisition process requires that resources and goals be brought into realistic agreement. Investing in technology, to offset significant numerical force disadvantages, has served our security interests well. This investment strategy is even more important in view of current Soviet advances in military systems technology. Key to the continued success of this strategy is the idea that our defense acquisition process must maintain a high-technology focus with the understanding that future strength through advanced technology development is as important as maintaining current strength through developed technology applications.

Ensuring future strength, through advanced technology development, requires a long-term commitment to steady investment in the science and technology base. Sufficient resources must be invested to ensure that advanced technology will be available to support future innovative operational concepts providing for the

wide range of contingencies envisioned by the <u>Discriminate Deterrence</u> strategy. Only by understanding our security goals and ensuring that the investment required to realize these goals is provided, will we ensure our future as a world leader into the next century.

The defense acquisition process must reflect these advanced technology development requirements better than it has in the last decade. Science and technology development managers must understand potential DoD weapon system requirements. Advances in computer gaming and simulations should be exploited to assist in determining these potential requirements. Extensive analyses of the various threats and possible future technology applications to address the identified shortfalls will be required. Integrated high-level gaming procedures and engineering simulations of the resulting weapon system possibilities can then be used to provide early net technical assessments supporting further advanced development.

Once we understand potential technology requirements, we must ensure that we invest intelligently and adequately in the supporting science and technology areas. This investment and subsequent development will result in advanced technology for introduction into future weapon systems. This effort must be supported by the development of an innovative, balanced risk-taking management environment ensuring that advanced technology alternatives are considered and pursued in weapon system development programs.

Finally, adequate Category 6.3A funds must be available to ensure the smooth introduction of technology into required weapon systems. Advanced technology integration into fielded defense capability will be enhanced through the development of major weapon system platforms with modular, upgradeable subsystems. This designed-in modularity, supported by standardization of

engineering practices, will provide the capability for recurring technology updating of major weapon systems through block change upgrading.

All of these recommendations require that attention be focused on the most essential element of all--people. The Department of Defense must be able to attract the best technology development management people in the country to ensure that advanced technology is identified, developed, and integrated into weapon systems supporting operational concepts.

B. MANAGEMENT RECOMMENDATIONS

To ensure that the science and technology base is successful in providing the Department of Defense with new advanced technology to support future operational concepts, the Working Group recommends the U.S. take the following actions:

- Move decisively to restore trust between the DoD and its contractors; they should be partners in planning and solution development, yet maintain an arms-length relationship during acquisition program structuring, contract competition, and award

- Develop a partnership that facilitates an early interchange between technologists and operational concept developers at the various DoD commands to seed innovative concepts for technology integrated with novel operational concepts

- Develop a method that facilitates early and open interchange between technologists, operational commanders, DoD acquisition executives, and key Congressional committees to establish a consensus on

reasonable goals and militarily useful specifications early in the acquisition process

- Foster an environment of balanced technology risk-taking, where appropriate high-risk and potentially high-payoff technology is considered and developed

  - Rein in bureaucracy in the acquisition process by reducing the number of program overseers during the early technology development phases of the program

  - Develop system requirements in terms of performance specifications and allow the development community to determine the design specifications

  - Develop management structures and procedures providing for greater visibility, accountability, and responsibility for top DoD executives ensuring that innovative technology is being developed and integrated effectively

- Develop a system of executive accountability necessary to successful management of technology

- Continue to build on current efforts to involve the Commanders-in-Chief (CINCs) in the process of developing requirements and priorities for new weapon system development programs

- Develop the ability to move high-leverage advanced technology into fielded systems more rapidly (an acquisition fast track) by setting priorities and enforcing these priorities in program budget decisions

- Develop major weapon system platforms with modularized subsystems to facilitate upgrading the latter every 5 years or so, as technical advances and program resources for such upgrades become available

- Develop a set of consistent and useful technology prototyping concepts to facilitate management of the development process

- Review the possibility of upgrading a modest number of essential key executive positions to attract world-class technology experts to the DoD, as was done during the 1950s, 1960s, and 1970s.

1. **Foster Partnership in Technology Development**

Partnerships in technology development are essential if innovative, advanced ideas are to be developed into advanced technology. These partnerships should be explored across the entire technology development spectrum from university laboratories, to operational concept developers, to industrial contractors, to the acquisition executives in the DoD, and ultimately to the Congress. The first two management recommendations are related and may be achieved through a more aggressive attempt to develop these partnerships.

It clearly is important and essential for DoD to maintain an arms-length relationship with industry in the contracting process. Nonetheless, the DoD needs to develop a procedure through which industry can actively participate in the development of emerging advanced technology and the concepts for the application of such technology to military problems. Service personnel who are responsible for the development of system concepts can benefit from close interaction and partnership with technology specialists in industry. Further, strengthening of

industry independent research and development (IR&D) by simplifying its management to get more creative contributions from the IR&D effort should be explored. The Working Group believes that many major military problems can be solved through such a partnership. This can be achieved through a combination of leadership, improved consensus between DoD and the Congress, and the appointment of highly qualified people to key acquisition positions.

This technology partnership should also involve the operational concept development commands in the various Services. Operational requirements must challenge the technology development community, and technology opportunities must challenge the operational concept developers. If this does not happen, we will remain unable to change the current trend of addressing only near-term problems, which require only current developed technology and result in only incremental operational improvements.

Operational concept developers need to be active partners in the early stages of technology development. Often technology still being explored in the laboratory can trigger new conceptual ideas that can, in turn, result in new operational concepts for that technology. An active partnership at this level will greatly enhance the probability that new technology emerges to support new operational concepts, leading to the maximum payoff from our science and technology investment.

2. Ensure Early Coordination and Consensus Development

To ensure the smooth transition of technology from the laboratory to a fielded system, a better consensus on the direction and requirements for the system must be developed early. The operational concept and the supporting advanced system technology should be understood, and agreement should be

reached between the technologists, operational commanders, Service staffs, DoD acquisition executives, and the key Congressional committees early in the advanced development period.

Consensus on the goals and reasonable, militarily useful specifications will remove the current non-productive, and often disruptive, procedure of annually defending all programs in the budget process. Continual uncertainty about the year-to-year viability of a development program is disruptive not only to the smooth transition of technology into fielded capability, but also to the people responsible for the success of the program. If this problem remains unresolved, it will continue to be difficult to attract and maintain the caliber of people required to ensure the successful development of innovative, high-risk technology.

3. Foster an Environment of Balanced Risk-Taking

The development of innovative, advanced technology is a long process full of uncertainties. If we are to have any success in developing technology that will ensure our role as a world leader and maintain our national security into the next century, we must be willing to take risks and expect and accept occasional failures in our technology development process. To develop and foster an environment in which risk-taking is acceptable, we must identify and change the elements in the process that have led to the current risk-averse program development climate.

The current adversarial relationship between the Congress, the Defense Department, and the defense industry has led to a large acquisition bureaucracy that is counter-productive. The requirement for program managers to ensure the completion of programs on-time and within budget, while satisfying the multitude of overseers, may be appropriate for large production contracts of proven technology systems, but is inappropriate

during the early stages of advanced system technology development. As discussed earlier, consensus on goals and reasonable, militarily useful program requirements for development programs should be achieved early. The program manager should work within this consensus and be unencumbered by competing bureaucratic requirements. If the consensus contains an adequate understanding of the risks involved in the development of the associated technology, the program manager should be supported whenever unavoidable overruns and delays occur. We must be willing to accept more risk early in the development program, when flexibility is needed and innovation essential. During this phase of development, fiscal exposure is relatively small and can accommodate risk-taking, while later, after development is successful, a lower risk management approach is warranted.

Service-generated program requirements currently not only define performance characteristics of the desired system, but also give detailed specifications on how the system should be designed and manufactured. This approach tends to produce risk-averse technology solutions and must be changed. Program requirements that define only the performance characteristics of the desired system will allow the technology development community to explore competing technology alternatives, yielding a best technology approach to providing the desired capability.

The Department of Defense should create a top executive position to emphasize technology considerations to all parts of the DoD management. This executive would be the DoD expert for the application of science and technology to all DoD problems. It would be this person's responsibility to ensure that emerging, promising technology is considered in development programs as well as in solving operational problems. This executive would also be responsible to the Secretary of Defense to ensure that program managers inject advanced technology aggressively into new

defense systems. This person, properly aided by a support staff, would be the critical link in creating a risk-taking environment within the technology and system development communities.

4. Develop a System of Executive Accountability

A particularly important ingredient in successful management of technology is accountability. Today, for all intents and purposes, there is none. The major program decisions--or milestone decisions--are made by committees (ultimately, the Defense Acquisition Board). Both the authorization and appropriations committees of Congress are heavily involved in program decisions, yet it is impossible to assess accountability for specific language in a given piece of legislation. Perhaps the greatest offenders are the Services, themselves. Program managers are repeatedly promoted out of their job. In a major weapon system program lasting 12 to 15 years, it is not unusual for the program director position to turn over 5 or 6 times. In any postmortem examination of why a particular weapon system failed to achieve some or all of its intended goals, it is virtually impossible to trace accountability for key decisions.

An obvious step toward resolving this situation is to commit the program directors to a longer tour of duty. A fixed period of time is not nearly as important as is the phasing of the program. For example, one director should carry a program from its inception through to completion of full-scale engineering development (FSED). If career considerations dictate a changeover, the new replacement should have a year or more as understudy before taking over the reins, and then should stay onboard for a major portion of the production phase.

Accountability is less easily assured in the OSD and Congressional decision process. A system should be devised that will identify a specific individual with key decisions,

particularly if those decisions alter the course of an acquisition program. For example, the 50 percent reduction in the procurement of Peacekeeper missiles has had major ramifications in every defense sector from strategic deterrent effects to the unit cost of procurement. Yet, it is highly unlikely that an individual decision maker could be identified with this decision. Without arguing the merits of the Peacekeeper case, per se, it is highly likely that greater consideration would have been given to the decision if an individual or small group were necessarily held accountable for the consequences.

5. Develop a Fast-Track Acquisition Process

When new advanced technology will clearly provide a significant military capability, it should be incorporated into an accelerated acquisition program. This type of program should have the coordinated support of the operational commanders, senior defense acquisition executives, and the Congress and be designated as a priority program. This priority should be reflected in the overall defense acquisition strategy and be enforced in all program budget decisions.

6. Develop Major Weapon System Platforms with Modularized Subsystems

A major problem in technology integration is the phasing of development and manufacture of major weapon system platforms, such as ships, tanks and aircraft, and their associated subsystems, such as armaments, communication systems, power plants, and sensors. While the platform can take as long as 10 years for its development, many of the subsystems can be available much more quickly. If development of the platform and the subsystems begins simultaneously, as is currently the practice, the weapon system is fielded with subsystem technology

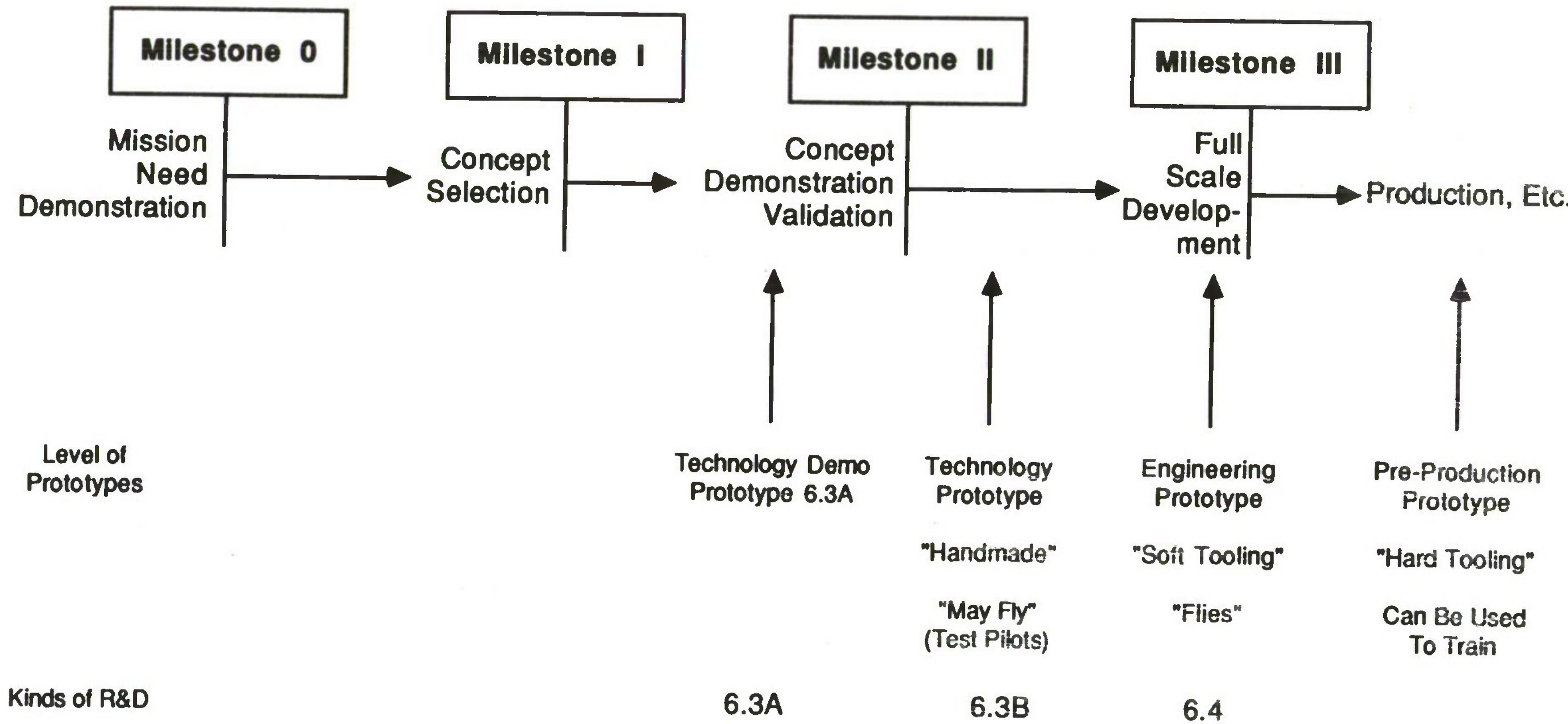
already 5 or more years old. Better development phasing would ensure that when the system is fielded all subsystem development would be completed within the same time frame, providing the best technology available. Subsystem modularity also protects the investment made in the weapon system by providing for easy retrofit capability, incorporating subsystems using advanced technology as it becomes available.

7. Devolop a Consistent and Useful Prototyping System

Prototyping provides the ability to explore several technology alternatives during the system acquisition process. A sound, consistent prototyping discipline helps ensure that the best technology solution is selected. But prototyping has to be more explicitly organized and systematically managed than is currently the case. Although there are a great variety of prototypes executed, each for different purposes, there is little or no coordination. The Packard Commission report recommended that DARPA become more involved in prototyping. This is a sound suggestion; however, until the various classes of prototypes are more clearly specified, funds made available, and military users support their development, this useful concept will not work well.

A potential solution to the prototype confusion is to tie prototype development to system development milestones. (See Figure 6.) For example, prototypes prepared for Milestone II would be technology oriented, but lack most manufacturing information, and would be operated only by technical experts. Prototypes prepared for Milestone III would meet much tougher criteria of documentation, manufacturability, and so on. Each level of prototyping needs to be clearly defined, balancing solid performance requirements with the maximum freedom to innovate consistent with the purpose of the prototype.

**FIGURE 6:
THE ACQUISITION PROCESS AND PROTOTYPING**



Clarification of the different classes of prototypes would also aid in deciding to stop development after a given level of prototyping. Such a decision could arise for many reasons, such as the bypassing of a technology by a new one, changes in threat, changes in military objectives, or budget shortfalls.

8. Attract and Retain Highly Experienced Technology Development Managers

A critical management crisis is facing the DoD: the increasing difficulty of attracting world-class technical executives into critical management positions. Only a few, perhaps less than a hundred, positions are involved, but in order to meet future technology and system development needs these positions must be filled by above average people. There is no substitute for the experience such top-quality technical executives would have with large, technically sophisticated industrial and military programs.

The most talented executives in industry, who are most qualified to fill these positions, have made this their life's work and are extremely competent. Accordingly, their compensation in industry is often 2 to 3 times the top salaries of Government employees. Furthermore, when they enter Government (if they do so at all), it commonly is for only 2 to 4 years, after which they return to careers in industry or academia. Current conflict-of-interest rules, as interpreted by the DoD, restrict such people from reentering their profession for 2 years after they leave Government. This is a major barrier to their recruitment.

The solution will be difficult. Conflict of interest, in fact and in appearance, must be avoided. Previous rules, which prevented the former Government employee only from representing his company to the government, were vague, but they can be

improved upon, made clearer and more effective, without precluding the service of talented individuals from industry.

The salary problem is a persistent one, but we need a few "super-super grade" positions to manage the complex business of identifying, developing, and deploying advanced technology. The Working Group realizes that it will be difficult to get Congressional authorization for such positions at competitive salaries; however, it recommends that the approach implemented by the National Institutes for Health and other Government agencies be reviewed for incorporation into the defense technology acquisition organization. A recent Defense Science Board study supports this compensation approach.

C. CONCLUSIONS

The effective identification, development, and integration of advanced technology is the key to providing a high-quality national defense capable of meeting the wide range of contingencies presented in the Discriminate Deterrence strategy. This is clearly an achievable goal. Technology development is a long and uncertain process requiring a clear understanding that future strength through new technology development is as important as maintaining our current strength through the fielding of developed technology systems.

The Technology Working Group's management recommendations can be summarized in five points. First, we must devote resources in realistic agreement with our objectives through a long-term commitment to stable funding. Second, system development goals and priorities must be understood and supported. Third, advanced technology must be incorporated into weapon system development programs through balanced risk-taking and a management approach embodying an increased awwareness of technology. Fourth, a more complete integration of advanced

technology development and associated applications must be accomplished throughout the acquisition process. And finally, world-class technology development executives must be attracted to the DoD to manage this process.

APPENDIX A

THE R&D PROCESS AND WEAPON SYSTEM ACQUISITION

Dr. Charles M. Herzfeld

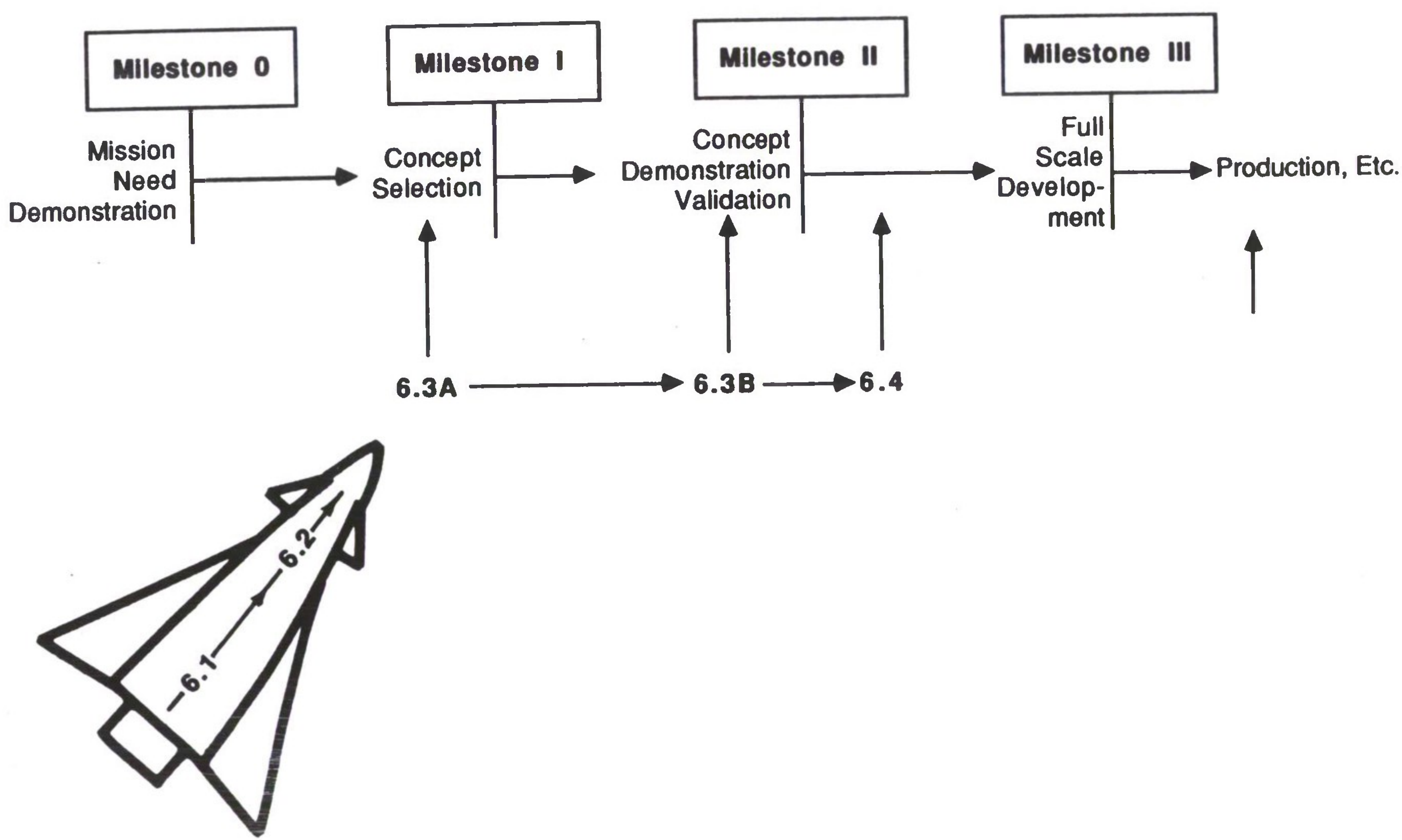
The purpose of this appendix is to provide a generalized overview of the Department of Defense acquisition process. While the details of any particular program may vary, the process outlined in this appendix will provide a framework for a general understanding of the acquisition process.

The acquisition process is designed to facilitate the orderly and systematic development of technology and its integration into fielded defense capability. While a standard approach is clearly defined, it is a characteristic feature of the process that it can be implemented in different ways. These implementations of the R&D process vary from the orderly, step-by-step systematic process, with a degree of predictability (referred to as the standard process), all the way to a highly opportunistic, "leap-ahead" process (referred to as the fast process). Between these two extremes, various mixed strategies are feasible. This variety of possible approaches to R&D complicates the management problem, making solutions difficult to develop.

The standard R&D process, (see Figure A1), begins with basic research in physics, chemistry, mathematics, and any other relevant field. Funding for this level of research is referred to as 6.1 funds (from former Secretary of Defense Robert McNamara's "Package Six, R&D, category 1, research"). Research for 6.1 activity is primarily done in universities, with some work in government and industrial laboratories. This research generally concerns the basic laws of nature and properties of materials, the mathematical consequences of logical conjectures, etc., to support further technology development.

The second R&D phase, exploratory development, 6.2, consists of applying basic science to concepts that could potentially be integrated into military systems applications. It must be understood that 6.2 development is exploratory in nature without being constrained to a specific system application. For example, a few lasers, emitting very stable waves of light, some optical fiber, plus special detection and signal processing equipment could make an exceptionally sensitive and accurate accelerometer for missile guidance and aircraft navigation. This specific example is an idea that was first considered about 20 years ago. While it was clearly a desirable technology for system integration, it took 20 years to develop, because it turned out to be quite difficult to integrate into a system application, even though the science was well understood.

FIGURE A-1:
THE R&D PROCESS AND THE AQUISITION PROCESS MILESTONES



This exploratory development phase of R&D is where the real inventing of new technology happens, and the feasibility of the new concepts are proven. Most of this work is done in industry and government laboratories, some in universities. While the concepts for implementation are often quite obvious, the success comes from developing ingenious ways to realize the concept.

The third phase, advanced development, 6.3A, consists of working out the best and most economical ways to implement the concepts invented in 6.2. This development activity is done before a decision has been made by DoD to field the concept. Concepts developed in 6.3A are risk venture concepts that may or may not be used. Development for 6.3A activity is done in industry and Government laboratories, with a small effort in universities (usually in Federal Contract Research Centers like Lincoln Laboratories).

The fourth phase, 6.3B, is also called advanced development, and consists of work performed after a decision to field the equipment is made. It is normally done in industry, and focuses on risk and cost reduction.

The fifth phase is full-scale development, 6.4. During 6.4 development, the final design of a weapon system is carried to completion. This phase is always done in industry, and is followed by manufacture of the systems. Figure A1 summarizes the R&D steps and their relation to the management phases in the DoD acquisition process.

There are alternative ways of developing systems that differ from the standard process described above. Many times technology developed through the 6.2 process will yield many system applications over several years. In these cases development programs will start at the 6.3A level and rapidly lead to the 6.4 development. While there is not a formal fast process, technologies that will clearly solve an important military problem can be developed and fielded quickly. In this process, when technology is developed and identified as the solution to a well-defined military problem, some of the development process could be waived or compressed.

APPENDIX B

THE STATE OF KNOWLEDGE IN THE BASIC SCIENCES

Dr. Hans Mark

By understanding the current state of knowledge in the basic sciences, we can begin to identify areas which could yield new advanced technologies and identify areas where more effort or funding may be required. This appendix looks in some detail at the four basic physical science research areas identified in chapter V, and their relationship to future technology opportunities.

The emphasis in this appendix is on the physical sciences and their likely contribution to national defense. Other fields, particularly those related to modern biology, will undoubtedly make major contributions, but the Working Group has found it more difficult to be specific about them at this time.

The state of basic knowledge in these scientific disciplines and, more important, the projected rate of change of that knowledge help select the technical areas with most leverage. It is not easy to answer these questions and many of the answers will be somewhat speculative. However, such speculations are undoubtedly of value in providing a framework for thinking about the problem and ultimately for making investment decisions.

Quantum mechanics has been a remarkably effective tool for 60 or more years, since the great contributions made by Heisenberg, Schroedinger, and many others in the 1920s. For all practical purposes, quantum mechanics is still the best theory for describing the behavior and structure of matter. It is remarkable that this statement is true for the lowest as well as the highest energy phenomena. Nothing has yet invalidated the basic theorems of quantum mechanics; that is, the uncertainty principle and the relationships between particle energy, momentum, wave length, and frequency have not been changed.

All discoveries that have happened in science in the last 60 years or more have yielded to understanding using quantum mechanics. These include lasers, the new high-temperature superconductors, the structure of proteins and nucleic acids, and the most exotic things in high-energy physics. The introduction of supercomputers has been particularly important. It is now possible to make ab initio calculations of chemical structures in complex molecules and predict their properties.

The field of atomic physics has been completely developed by the ability to calculate very detailed properties of atoms. For example, the development of x-ray properties of atoms has led to

the development of x-ray lasers driven by nuclear explosions. The likeliest break in this situation probably will come when we try to understand the properties of matter under extreme conditions in objects such as quasars or black holes. At that point, it is at least possible that the basic theorems of quantum mechanics will break down, and will be replaced by some generalizations of them.

The physics of the electro-magnetic field as first developed in the mid-19th century and then modified by the theory of relativity in the early years of this century, have also been remarkably durable and successful. Once again, scientific discoveries such as lasers, the properties of ionizing radiations, and the behavior of electro-magnetic waves used in the most sophisticated communication schemes have all been successfully described with the currently available theories. Complex problems in non-linear optics have yielded to a combination of what we know about electro-magnetic theory and the structure of matter using quantum mechanics.

Once again, the likeliest place to look for changes in this situation is in cosmology, where matter exists under particularly exotic conditions. There may also be some important surprises when an understanding of the propagation of radiation over great distances is achieved. For instance there are some who believe that the cosmological red shift is not a Doppler shift but rather a property of radiation itself. Should that be the case, then a breakdown of the conventional wisdom will have occurred, which in this case may or may not have practical consequences.

Transport physics has been a bit of a stepchild for the first three quarters of this century. It has definitely been overshadowed by quantum mechanics, nuclear physics, particle physics, and other areas of modern physics. Recently, perhaps in the last decade or so, there has been a resurgence in this field. Transport physics is characterized by the fact that the Boltzmann equation--which is the fundamental law--tends to lead to highly non-linear differential equations in practice (the Navier-Stokes equation is a good example). Non-linear equations of this kind have resisted attempts to solve them in general terms until the recent advent of supercomputers.

It is the existence of these high-speed computing machines that has made possible really remarkable progress in transport physics. Perhaps the best example is the discovery of regularities that have turned up in computer solutions of turbulent flows that have led to insights into the nature and statistical features of turbulence. This has come to be called Chaos Theory, and it is perhaps the most important example of how numerical solutions using high-speed computers can lead to new fundamental knowledge.

The same statement can be made for plasma physics, where highly non-linear equations also govern the behavior of these ionized gases. Finally, energy and radiation transport have also benefited in important ways from the availability of very high-speed computers.

Mathematical physics is probably the fastest moving of the areas in basic scientific knowledge listed in this report. The advent of high-speed supercomputers has been instrumental in all recent discoveries in this area.

The technology of computers is still improving. One reason for this projected improvement is that new developments in electronic technology will provide computer architects with faster and smaller components. Perhaps more important are research results in computer architecture and computer design. Advances in this area come from new knowledge in fundamental mathematics, including topology, geometry, advanced statistics and even more fundamentally, the theory of knowledge itself.

It is the development of new computers that probably will have the most important impact on fundamental scientific research in the coming decades. There are new developments on the horizon in computer design involving new organizational principles for computing machines that will change entirely the way we do business in this important area. The United States still has a major advantage in this field. This leadership is being challenged by the Japanese who have caught up with the United States in the development of computer architecture, design, and software.

While the health of basic science in the United States is good, a word should be said about the administration and funding of basic research. Judgments on funding distributions are made today primarily using a peer review system, in which committees of the National Academy of Sciences, the National Academy of Engineering, and of the various funding agencies in the Federal Government make the fundamental funding decisions. This system generally works well, but suffers from a major problem. Specifically, it is often difficult to get radically new work funded because the peer review committees tend to be dominated by people who have helped to create the currently accepted structure of scientific knowledge. These people are often not friendly to having this structure disturbed. Thus, genuinely new ideas tend to be short-changed when funds are distributed by a peer review mechanism.

There are some excellent examples that illustrate this point. Professor Richard Mueller of the University of California (Berkeley), for example, was unable to get funding through the normal channels for an experiment that eventually turned out to

be most fundamental in understanding the nature of the background radiation in the universe, the so-called 3°K radiation. He had to rely on funding that was arbitrarily granted to him by the director of NASA Research Center. Professor Mueller subsequently won a MacArthur Foundation prize for this work and his results are now well accepted. Some research funds must be available outside the peer review system in order to take care of exceptional cases.

MAJOR TECHNICAL AREAS LIKELY TO DOMINATE FUTURE TECHNOLOGIES

A discussion of progress in basic science is interesting, but it is not useful for technology assessment until it can be applied to practical ends. There are 10 major technical areas that are important for national defense and that also have clear importance to national economic development. It is useful to list these areas and to show how they are related to the basic scientific disciplines on which the technical areas are based.

The connection between scientific progress and technical progress is a subtle one and the implication that the two are directly connected is not necessarily correct. There are many other factors such as the rate of investment in various technical developments, the availability of people to carry them out, and the general atmosphere that determines what interests people affecting the outcome. Nevertheless, history has shown that the connection is so clear that it needs to be taken very seriously.

There is no doubt, for example, that the development by Maxwell of the theory of electro-magnetism in the 1850s led rather directly to the creation of a strong, electrical industry in the ensuing 2 decades. Likewise, the early basic research in organic chemistry by Emil Fisher and his colleagues in the last 2 decades of the 19th century, initiated the strong German chemical industry in the early years of this century. Closer in time and geography, it is rather clear that the discovery of the transistor at Bell Laboratories by Bardeen, Schockley, and Brattain in 1950, followed by the 12 national materials research centers established by the Advanced Research Projects Agency and the Atomic Energy Commission at good universities in 1960, pushed the creation of the U.S. semiconductor and computer industry, indeed made its growth possible.

THE KEY TECHNOLOGIES

Once again, as in the case of the scientific disciplines, there is considerable overlap between the major technical areas, and the definitions that have been made are, to some extent, arbitrary. However, it is probably not too important to consider the exact scheme that has been used to categorize the various

scientific and technical efforts. The important point is that there is some order around which thinking about the problems can be organized.

Listed below are the 10 key technical areas of interest. The basic disciplines on which progress in each of the technical areas depends are noted in parenthesis next to the technical area.

- Materials technology. (Quantum mechanics, particle physics, computational physics.)
- Electronics, computer, and electrical technology. (Electromagnetic theory, quantum mechanics, computational physics.)
- Nuclear technology and nucleonics. (Transport physics, quantum mechanics, particle physics, computational physics.)
- Plasma technology. (Transport physics, electromagnetic theory, computational physics.)
- Optical technology. (Electromagnetic theory, quantum mechanics, computational physics.)
- Energy technology. (Transport physics, electromagnetic theory, quantum mechanics, particle physics, computational physics.)
- Fluid mechanics and aerodynamics. (Transport physics, computational physics.)
- Biotechnology. (Quantum mechanics, electromagnetic theory, computational physics, transport physics.)
- Biomedicine. (Quantum mechanics, transport physics, computational physics.)
- Manufacturing technology. (Robotics, design for simplicity and quality, testing and inspection, management for quality.) This is somewhat different from the other technologies, but equally important, and relatively neglected in the United States.

What is most important about this list of major technical areas is that all of them depend on progress in computational physics. This is a point that must be stressed in any strategy for technical development. It is very likely that in the next 2 or 3 decades progress in any of these technical areas, at the

basic level, will depend on progress in computational physics, and therefore, on the rapid development of computer technology. Basic research investments in this technology area are likely to yield important benefits across the board. This is particularly true of basic work in the mathematical disciplines related to the organization and architecture of computers. There are many who believe that the development of parallel architecture, hypercubes, connection machines and other new architectural concepts in computer science and technology will lead to enormously important breakthroughs in other areas as well.

IMPORTANT APPLICATIONS

The state of knowledge in the scientific disciplines described above seem to be vital for the steady generation of new technology required for national security. It is important to show how these scientific opportunities are likely to evolve into high-leverage technologies. When dealing with the question of practical applications, the most important investment decisions must be made.

Materials

The most important development in the last 20 years in this technical area is the creation of tailored or engineered materials for many purposes. This broad area of synthetics ranges from new plastics to sintered metal alloys created by new techniques in high-current electrical technology. The most promising applications include:

- **High-temperature, high-strength materials.** Single crystal turbine blades, exotic sintered alloys as applied to armor for tanks, and materials for the development of gun barrels, the construction of submarines and aircraft and many other applications. Much progress can be expected in this area and it is clearly important to the national security because it will make possible aircraft, ships, and tanks with much higher performance.

- **Structural composites.** The important considerations for primary structural materials for aircraft and spacecraft are light weight and high strength. Improved armor for aircraft and for other vehicles can also be expected. The technology is well advanced, but much more progress can be expected. Very important national security-related applications are possible.

- **Materials with special electrical and electronic properties.** Composite materials for the construction of stealth vehicles and other low radar, cross section applications. High-temperature superconductors, a genuine recent surprise that clearly has many important defense applications. Materials with especially high dielectric constants that can be used as energy storage media in capacitors making more feasible directed energy weapons of all kinds. Much more progress in this area can be expected, driven by obvious defense requirements.

- **Fire-resistant material and paints.** Polybismailides and other synthetic materials with fire- and heat-resistant properties. They are very important for shipboard and aircraft applications to prevent fires and to contain fire damage. Synthetic foams for use inside fuel tanks to prevent gasoline fires on aircraft can be developed. Much progress in the development of materials of this kind can be expected and the defense applications are obvious.

- **Solid lubricants.** It is now possible to implant lubricating materials like graphite in the structural elements or the moving parts of an engine. As the moving part of the engine wears, the lubricant is metered out in precisely the right quantity so that no lubricating oil from the outside is necessary. The application of this technology could greatly reduce engine maintenance costs and increase reliability.

- **Active synthetics.** These are materials that change their properties as a function of external conditions for various purposes. Optical materials, for example, react to light intensity. Medical applications will emerge: capsules that are either swallowed or implanted can be designed to deliver drugs to people in a precisely metered fashion. Much progress can be expected in this area.

- **Substitute materials.** Shortages of various strategic materials have been thoroughly documented. Many of these shortages can be mitigated or eliminated by the development of substitute materials. Research in this area should be given the highest priority because of vital national security applications.

Electronics

Important advances are to be expected in the areas of electronic components. It will be possible to reduce the size of

electronic components by about a factor of 100 before the physical limitations imposed by the rules of quantum mechanics for the functioning of semiconducting switches are reached. The argument for this prediction is based on fundamental scientific considerations and is very likely to be correct. Some promising applications appear in the following list:

- Integrated circuits. More advances in the manufacturing of integrated circuits are on the horizon. Electron beams and x-ray techniques for making mats will make it possible to build smaller circuits. Smaller integrated circuits are of obvious importance for the development of smaller, more compact and higher performance computers. Developments in this area are, therefore, critical to achieving some of the objectives that have been stated with respect to the application of computational physics. National defense applications such as super smart munitions are of the highest priority.

- Sensing devices. Detectors for low-level photon fluxes at all frequencies. Electro-acoustical devices (important for antisubmarine applications) and very sensitive strain gauges all are in a rapid state of development. Most of these devices depend on the understanding of electro-magnetic and electro-optical properties of materials. Applications range from the detection devices on satellites designed to look at rocket launches to sonar devices used in antisubmarine applications.

- Electromagnetic materials. Piezoelectrical materials in optics, and polarizable materials for energy storage in capacitors are examples, as well as high-strength materials for energy storage and rotating machines such as homopolar generators. Both the polarizable materials and the high-strength materials for energy storage devices have important potential applications. In the case of rotating machinery, energy storage on the order of 10 percent of the value of chemical high explosive can be achieved in the next few years. There are obviously important military applications of this technology including the development of electro-magnetic hypervelocity guns. Such guns would lead to the creation of artillery that does not have the limitations of "bullet" velocity imposed by the use of chemical explosives as propellants.

- Computer design. This is closely related to electronics, but covers a whole range of new ideas. Computers will be both faster and smaller in the future

and this technology is a central component in all other matters discussed in this report.

- **Communications technology.** New microwave devices will be developed that will make high frequency communications in the 20 to 30 gigahertz region possible. This will be especially important for satellite applications. Laser communication experiments in space have already been conducted and have proven the feasibility of this technique. Fiber optics is another technology that has just begun to appear in the first important applications. Defense applications continue to be important.

- **Radar technology.** Important improvements can be expected due to better electronic components and computers. Particularly, the special applications such as, space-based radars for surveillance and targeting and terrain-following radars for aircraft and cruise missiles are most promising for further progress. Imaging radars, because of their ability to penetrate cloud cover and to be used at night, will find increasingly important military applications.

Nucleonics

In spite of the slowdown in some areas of nuclear technology in this country, there are, nevertheless, interesting developments in nucleonics that are very likely to have important military applications. Items of particular interest are:

- **X-ray lasers driven by nuclear explosions.** This achievement was possible because of new understanding of atomic physics, that is, the physics of highly ionized atoms made possible by the advent of supercomputers. X-ray lasers could have most important applications in strategic defense and other areas requiring very long-range weapons but practical applications are more than a decade in the future.

- **Special purpose nuclear weapons.** Many discoveries are happening in nuclear weapons design, again aided by the development of very high-speed computers. The results are classified so it is not possible to describe them here. The major point is that the design of nuclear weapons and nuclear devices is not a finished field; many important improvements are possible.

- **Nuclear reactor design.** The development of efficient gas-cooled reactors in the last 10 or 15 years may be a promising way out of the current dilemma faced by the nuclear power industry. Gas-cooled reactors are inherently safe, and they are more efficient than water-cooled reactors because they operate at higher temperatures. Gas-cooled reactors are a very promising choice for the next generation of nuclear power systems. Military applications are secondary.

- **Space nuclear power.** The Russians have developed nuclear reactors to power spacecraft. The United States has not had an equivalent effort. For spacecraft or space stations that require continuous power delivery, above one megawatt nuclear reactors are required. Space nuclear reactors are essential for applications of space-based strategic defense systems. The current project to develop space nuclear power reactors (SP-100) should be pushed with all deliberate speed.

- **Isotope applications.** Applications of isotopes continue to grow, especially in applications to radioactive tracer techniques. These have proved critical in developing structural analyses of complex biological molecules such as proteins and deoxyribonucleic acids. Very important for basic progress but not directly related to military applications.

- **Nuclear fuels.** There is no shortage of nuclear fuels in the world. The United States has adequate resources. Eventually, these will be developed intensively in the United States as other power sources decline, for environmental or fuel shortage reasons. The nuclear industry must maintain its readiness for rapid expansion. Military applications for nuclear submarine reactors require the maintenance of an adequate nuclear fuel supply.

- **Nuclear waste disposal.** The technology for nuclear waste disposal is in hand. The location of the high-level nuclear waste disposal site will be a high-priority political decision for the next administration. The military importance of this problem arises from the need to dispose of wastes from nuclear submarine reactors.

Plasmas

Plasma physics is one of the most difficult experimental and theoretical areas in modern science. It is also one of the oldest, since modern physics grew out of the early experiments with gas discharges. Finally its applications are extremely versatile, ranging from arc welding to the experimental work on nuclear fusion, to plasma measurements in the upper atmosphere. The most important applications are listed below:

- **The structure of the upper atmosphere.** A detailed understanding of the chemistry of the upper atmosphere is critical for dealing with situations such as the ozone problem. Understanding the physics of the upper atmosphere, especially the location and behavior of charged particle layers or plasma ducts, has critical applications to communications and to radar technology. Long-range, over-the-horizon (OTH) radars that use reflections from the upper atmosphere have particularly important military applications. Predictive theories of the behavior of the upper atmosphere are needed to use over-the-horizon backscatter (OTH-B) effectively.

- **Fusion energy.** This is the classic area of high-temperature plasma research. Recently, a decision has been made to concentrate on the Tokomak geometry for fusion research. It is not clear whether this is wise. The Tokomak geometry is probably the best, but this is still not a sure bet. The development of fusion energy is surely important for the long run. After 30 years, we are still looking for the way to do it, and this has been discouraging. The other side of the coin is that we have not found any particular reason why fusion energy cannot be extracted from the DT and the DD reaction in a controlled way. There is no scientific law that says it cannot be done. Fusion research should be continued at a good level of support to help meet long-term energy requirements.

- **Space propulsion.** The requirements for low-thrust, high specific impulse propulsion are important for a number of space applications. Plasma jet propulsion is a good candidate. The long-term future requirements are for strategic defense, a return to the moon for the establishment of a permanent base, and eventual trips to Mars. Flight experiments on a small scale should be performed.

- **Magnetohydrodynamic energy** conversion. This technology could be important for space applications related to strategic defense and other large-scale power

operations. Ground-based research is required before any commitments to space-based systems can be made.

- **Welding techniques.** High pressure plasma arcs are used as the environment in which the welding of large blocks of metal can be accomplished. This has obvious applications to the welding of large diameter pipes, armor plate metal processing and other manufacturing technologies. Some basic knowledge of plasma behavior has been important in the development of this technology and work in this area should be continued for possible military applications in manufacturing.

Optics

The science of optics has seen great strides in the past 2 decades. The advent of lasers in 1962 probably was responsible for the resurgence of optics. Physical optics, the interaction of materials and electro-magnetic radiation, has seen a particularly important renaissance. Within this area, the non-linear interaction between radiation and matter has led to both interesting and important applications as listed below:

- **Gas dynamics and chemical lasers.** These lasers depend on high-energy gas flows and chemical interactions to produce power. Lasers with power outputs of 1-10 megawatts have been built using these principles. Gas dynamic and chemical lasers are the most promising for near-term military applications.

- **Free electron lasers.** These are very promising devices for the creation of high-energy laser beams. Several have been made to work at relatively high power levels, but still at relatively low frequencies (microwaves). The principle of the free electron laser is different from other lasers since the energy of the laser beam is extracted from a high-energy free electron beam rather than from quantum levels of atoms in materials or gas flows. The principal advantage of the free electron laser is that it can be operated at any desirable wave length and has the potential for very high energy output. The military potential is for ground-based, antisatellite weapons, as well as SDI applications.

- **Glass lasers.** Progress in high-energy glass lasers has been substantial. Great technical difficulties with the interaction between radiation and the glass in the non-linear region have been overcome. The applications

of these lasers are still in question and materials technology is clearly important in this area.

- **Laser propulsion.** This is a long-term possibility that has been suggested, and a few preliminary experiments have been performed. The potential is very interesting because it may become a very cheap way of lifting large amounts of material into earth orbit. It is not clear whether it will work, and therefore should be kept at the experimental level for the present.

- **Fiber optics.** Fiber optics have become very important for communications applications. This is in part due to advances in material technology that permit the tailoring of glass or plastic fibers so that they have good light transmission properties. The use of light for information transmission makes broad-band (high data rate) communications possible. Furthermore, fiber optic communication systems are nearly invulnerable to electro-magnetic interference. This last point is important for military applications because it permits hardened communication links. Optical fibers can also be used to build very sensitive detectors of a variety of physical effects, e.g., pressure and temperature. Fiber optics is a high-priority development area.

- **Adaptive optics and image compensation techniques.** There has been great progress in the development of optical systems that compensate for atmospheric disturbances. This is important both for laser beam steering devices and for telescopes used in astronomy. Adaptive optics as well as compensation techniques are absolutely necessary for the development of high-energy lasers that propagate beams over very long distances in the atmosphere. These technologies are, therefore, necessary for strategic defense and for other military applications.

- **Optical surfaces.** Mirrors for high-energy lasers must have special surfaces. Specialized coatings are necessary to reduce the heat loads imposed on the mirror material by the laser beam. Cooling techniques for mirrors must be developed as well. This is a difficult area but it is critical if high energy lasers are ever to be militarily useful.

Energy

Energy technology is fundamental because energy is the motive force behind both the economy and national security. Technologies discussed here relate to energy production transmission and natural resource recovery, which are important from a broad national security point of view. Some critical development areas are listed below:

- **Hydrocarbon fuels.** Conventional oil recovery, enhanced secondary and tertiary recovery, and the recovery of oil from shale must continue to have the highest priority since the economy and the military will be heavily dependent on oil for the foreseeable future. Work on shale oil is particularly important since the United States has huge shale oil resources. At the present time, shale oil recovery is not economical, but the investment in shale oil research and pilot plant development should be made, not on the grounds of economic benefit, but on the grounds of national security. This should have a very high priority.

- **Plant-based hydrocarbons.** There are some promising prospects of recovering oil from plants. This refers to the use of genetic engineering to modify certain trees that produce hydrocarbons naturally in such a way that the hydrocarbons can be converted to hydrocarbon fuel products. These trees are similar to rubber trees that produce rubber latex, which is a natural hydrocarbon. Plant-produced alcohols as fuel additives also constitute a promising approach to the development of new oil substitutes. Research in this area must have a high priority for both civil and military applications.

- **Deep gas recovery.** There are interesting prospects for discovering new reservoirs of natural gas at depths in excess of 15,000 feet. Several have already been exploited. There is, moreover, a theory based on what has been learned in the research on the evolution of the planets that suggest that very deep gas reservoirs may not be of biological but of geochemical origin. The reservoirs may accumulate gas from seepage of methane and other hydrocarbon gases produced in the earth's core, which seep through the earth's mantle. If this theory proves to be true, then the natural gas found at such great depths would be a renewable rather than a non-renewable resource. If verified, then very high priority should be given to the development of

deep drilling techniques for both civil and military applications.

- **Propulsion.** Turbines operating at very high temperatures have proved to be most important for aircraft propulsion. The development of ram-jets and hydrogen fuel turbines for applications to the aerospace plane are on the horizon. There are important potential applications in that area but they are rather long term. Turbine engines will also be used for other vehicles. Some ships, tanks, and automobiles are already driven by turbines, and more will be. Turbines must be operated at very high temperatures to compete with the efficiency of internal combustion engines. Cheap, high-temperature turbines for automobiles and land vehicles will probably require the use of ceramic metal composites that are now in the test stage of development.

- **Power plant technology and electric power transmission.** Slow and incremental progress in power plant technology is to be expected due to the availability of better materials. Perhaps the most interesting development in this area is the potential to use superconducting materials for electric-powered transmission. Transmission losses now account for 15 to 25 percent of the power losses between the producer and the user. Superconducting power transmission is, therefore, promising as an overall energy-saving technology. Here, the military applications are minor.

- **Natural resource recovery.** This is considered under energy, because it is an energy-intensive technology. The United States has some natural resource problems that have been well documented. Oil is the most important one. The strategic oil reserve and maintenance of the domestic oil development industry are critical to the energy posture of the United States. Strategic material shortages exist but these can generally be overcome by the development of substitute materials and by the use of known but uneconomic resources of the same materials in this country. Most of the U.S. strategic material problems are not due to shortages of raw materials in this country, but rather to the fact that these materials can be developed more economically overseas.

Fluid mechanics

There have been significant advances in fluid mechanics in the past decade because of developments in high speed computers. Some new physics in the atomic and molecular areas have also been important, particularly in the understanding of chemically reacting flows. Numerical solutions of non-linear differential equations with complicated boundary conditions have become possible. In addition, numerical solutions have led to new theoretical insights. Chaos theory and the macroscopic order phenomena in non-linear dynamics will lead to theories with good predictive capabilities in the next few years. Some applications of these are listed below:

- Airplane and ship design and development. Computers have become numerical wind tunnels. This has reduced the time and cost of aircraft design. This is also true about ships. Computers are numerical towing tanks in the case of ship design. Both of these clearly are extremely important for defense applications.

- Weather forecasting. Long-range, large-scale weather forecasting is now reasonably good. The results depend on having an accurate energy balance and good fluid mechanics equations with valid turbulence models to describe the atmosphere. Large-scale phenomena such as the jet stream, the trade winds, the more-or-less permanent low pressure areas over the north Atlantic and north Pacific can be predicted. It may be possible with better computers and some of the new theoretical developments in fluid mechanics to make progress in predicting smaller scale phenomena, such as hurricanes (scale about 300 miles), compared to those that can be done at present (scale about 1,000 miles). Further into the future, even tornadoes (scale less than 1 mile) and thunder storms (scale about 5 miles or 10 miles) may be predictable. Good weather forecasting is of obvious military value.

- Ocean engineering. This field is also strongly related to the understanding of fluid mechanics in the prediction of ocean currents, temperature gradients, and other phenomena of importance to understanding the properties of the ocean. Applications to submarine warfare are obvious. Ocean engineering is also important for resource recovery operations. Underwater drilling for oil and perhaps recovering other resources from the ocean floor may become important eventually from an economic viewpoint and perhaps also for military purposes.

- **Properties of matter under high pressure.** When materials are subjected to high velocity impacts, for example, the materials become fluid and the method of fluid mechanics must be used to determine their behavior. Shaped-charge explosives also fall into this category. The question of how armed vehicles and ships can be damaged (and protected) is strongly dependent on this technology. The military value is obvious and the priority is high because of recent Russian advances in armor technology.

Biotechnology

This is a field of great promise for the future. Biotechnology is the application of physical science and technology to biological problems. Because of the strides made in various physical sciences, a corresponding impact on biotechnology is to be expected. Some of these are listed below:

- **Human factors.** The behavior of people when dealing with machines is an important consideration in machine design. It is even more important in weapons systems because of the inherent danger of the situation in which weapons are employed. Weapons must be designed and built so that real people under great stress can operate them. This is an intellectual discipline that is sometimes ignored. It is a large and diffuse area, but more must be done to apply it intelligently.

- **Prosthetic devices.** This is perhaps the most promising field in biotechnology. The possibility of producing artificial limbs actuated by real nerve impulses and guided by advanced sensors and computers is close at hand. Aides for sight and hearing losses are in various stages of development. There are obvious military applications of these technologies to the treatment of casualties.

- **Interactive devices.** This term refers to devices that can be actuated by voice or stimuli other than hands, feet, or other mechanical means. There are, for instance, experiments on airborne fire control systems controlled by the movements of the pilot's eyes. Voice-actuated systems are also in the experimental stage, for use in situations where the workload on the operator is high. The defense implications are obvious.

- **Protective systems for troops against various biological warfare agents.** The United States is far behind the Soviet Union in this area and it is imperative that measures be taken to make up for the current disparities. This should be given an extremely high priority and the work should take advantage of what is happening in materials technology, biotechnology, and other fields.

- **Intensive care equipment for field hospitals.** This work parallels what has been achieved in civilian hospitals. It could be most important for minimizing casualties.

Biomedicine

Biomedical technology is different from biotechnology in that it depends on advances in medicine and biochemistry rather than in electronics and other physical engineering disciplines. Aside from the obvious military applications in the treatment of casualties, there are several other important considerations from a public health standpoint. The most important are listed below:

- **Toxic agents and nerve gases and their antidotes.** Much work in this field has been done. Molecular biology is critical to the understanding of the possible effects and countermeasures for such weapons. These countermeasures would include finding effective drugs to neutralize internal effects and agents to minimize problems caused by the exposure of the skin or the eyes.

- **Infectious diseases.** This is still a problem in regions where wars are fought. Constant and sustained efforts are needed in order to minimize casualties in the field. This is especially true after people are wounded.

- **New surgical procedures.** The treatment of combat casualties has traditionally led to the development of new surgical procedures for severe trauma cases. The advent of new physical techniques--lasers, for example, or cryogenic methods--are being exploited to expand the number of cases that can be resolved surgically.

APPENDIX C

THE COMING AGE OF STEALTH

Barry Watts

> Stealth is going to be an important part of the military balance of the nineties. Leadership in Stealth technology will be a substantial advantage, particularly in air warfare . . . The advantage of Stealth technology is that it thickens the (Clausewitzian) fog of battle, but--as long as leadership is maintained--it does so for only one side.
>
> -- Bill Sweetman[3]

The emergence of advanced aircraft that depend primarily (rather than secondarily) on stealth derived from low-observable technology (LOT) to execute wartime missions inevitably creates a requirement for equally stealthy mindsets on the part of organizaticns and individuals operating them. But such attitudes cannot be expected to occur naturally in military cultures where stealth has not been operationally important in the past. In such cases, the requisite mindsets usually need to be developed.

In the case of the U.S. Air Force, many of its most basic and enduring beliefs about the proper employment of air power still have strong linkages to World War II combat experiences with environments in which stealth was neither terribly important nor always desired. For example, the concept of offensive action emphasized by the U.S. Eighth Air Force against Nazi Germany consciously sought "to induce maximum fighter opposition on every mission launched."[4] In other words, this concept was far from striving to avoid pitched air battles against the backbone of the Luftwaffe's fighter defenses during the height of the struggle for daylight air superiority over Germany. The Eighth Air

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[3] Bill Sweetman, Stealth Aircraft: Secrets of Future Airpower (Osceola, Wisconsin: Motorbooks International, 1986), p. 93. The fog of battle is simply one component of Clausewitz's broader notion of general friction, which he characterized as "the only concept that more or less corresponds to the factors that distinguish real war from war on paper" (Carl von Clausewitz, On War, trans. Michael Howard and Peter Paret (Princeton, New Jersey: Princeton University Press, 1976), p. 119).

[4] William E. Kepner, Eighth Air Force: Tactical Development, August 1942 - May 1945 (England: Eighth Air Force and Army Air Forces Evaluation Board (European Theater Operations), 9 July 1945), p. 137.

Force's operations were designed to provoke the greatest enemy reaction possible. This strength-on-strength approach--however unavoidable at the time--was not only abetted by a greater capacity to accept attrition than Air Force leaders believe they possess today, but was virtually the antithesis of stealthiness.

There is little reason to believe that the attitudes of Air Force operators toward stealth have evolved much since World War II. Right down through the 1972-73 LINEBACKER operations against North Vietnam, massed formations of penetrating aircraft, active countermeasures, and strike-force packaging aimed at smashing directly through enemy air defenses to the target continued to characterize the dominant operational style of U.S. forces.[5] Similarly, even today, in theaters like Central Europe survival is still seen by American airmen mainly in terms of low-level penetration tactics, suppression of enemy defenses, and electronic countermeasures (ECM).

It may well be, then, that a major challenge for the U.S. Air Force in the coming Age of Stealth will be to nurture and institutionalize appropriately stealthy attitudes and mindsets. Among other things, the operational security practices, employment concepts, and tactics best suited to a world dominated by stealth are likely to differ substantially from those on which American airmen have rightly relied on the past.

Looking first at operational security, there appears to be a number of traditional Air Force practices that may have to undergo considerable revision if the United States is to sustain its early advantages in this new area of military competition over adversaries like the Soviets. If the Advanced Technology Bomber (ATB), for instance, is to succeed in imposing disproportionately greater costs and stresses on the Soviets, then the ATBs the U.S. begins fielding in the early 1990s will need to remain operationally effective relative to Soviet air defenses for 2 decades or more. Much the same can be said of the Advanced Tactical Fighter (ATF).

This requirement for the advantages conferred by low observability to be long lived immediately suggests one area in which traditional Air Force practices may have to be modified or changed for the Age of Stealth: the protection of raw platform signatures. Even in the case of friendly surveillance from

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[5] William W. McMyer, Air Power in Three Wars (WWII, Korea, Vietnam), ed. A. J. C. Lavalle and James C. Gaston (Washington, D.C., U.S. Government Printing Office, 1978), pp. 125-9 and 222-7. The resemblance between Eighth Air Force penetration tactics during the period 1943-45 and those used by U.S. Air Force F-105s and F-4s for daytime strikes in the "Route Package VI" region of North Vietnam in the years 1967-69 is uncanny.

domestic air traffic control radars, it would seem unwise to expose actual wartime levels of signature reduction on a daily basis; in international airspace or overseas, such exposure would appear all the more dangerous. So for purposes of peacetime flying operations with LOT bombers, fighters, and reconnaissance aircraft, the U.S. may want to make sure that only magnified or otherwise disguised signatures are exposed--the crucial point being that the necessary security measures need to be in place and relatively free of bugs right from the outset.

A more subtle change in the outlook of the Air Force operators that stealth may eventually necessitate concerns the patterns and practices of crews. In the case of a submarine trying to avoid detection by modern acoustic sensor arrays, an inadvertent noise created by a single negligent crewman in the course of performing normal maintenance on the propulsion gear could possibly result in detection. Similarly, recurring operational patterns analogous, for example, to the fighter pilot's mistake of always pulling off the target to the right could unwittingly aid enemy efforts to achieve detections. Thus, because detection is potentially so serious for any vehicle that depends principally on stealth to survive, all portions of the Air Force operating stealth aircraft will need an organized capability to detect, document, analyze, and prevent these sorts of avoidable breakdowns in operational security.

Finally there is the matter of anticipating enemy countermeasures to U.S. low-observable technology. The name of the game, once again, is staying ahead, and this imperative would seem to argue for not only creating, but also institutionalizing a U.S. "low-observable Red Team" the objective of which would be to devise effective countermeasures to our own stealth vehicles and, then, develop countermeasures. This charter might well include not only building and testing the most promising counters to platforms like the ATB that U.S. aerospace manufacturers could suggest, but doing the same with virtually any systems the Soviets appeared to be pursuing as well. Such a group should also subject our own operational patterns to the kinds of analyses we can expect the Soviets to conduct.

Turning from operational security to employment concepts and tactics, how might low observables eventually change the face of future aerial warfare--especially at the aircrew level? Here again, even rudimentary "blue-sky" thinking suggests that the Age of Stealth will demand ways of thinking and approaches that may diverge considerably from more traditional Air Force ways of flying and fighting. To begin with, the essence of LOT--reducing observable signatures in order to carry out missions undetected--runs counter to the Air Force's long-standing emphasis on centralized control and non-nuclear strike-force packaging. The idea, for instance, of packing large numbers of ATBs into World War II (or even Vietnam-era) daylight penetration

formations does not seem very conducive to remaining undetected. Similarly, would it make any sense to attempt direct-escort of relatively non-stealthy interdiction fighter-bombers like the F-16 with stealthy ATFs? In the case of the ATB, it would look preferable, tactically speaking, to operate the platform in very small formations or, better yet, as single ships. As for the ATF, the aircraft's inherent stealthiness may make mixed-force operations with current generation F-15s and F-16s extremely difficult to orchestrate--especially in a theater as crowded as Central Europe is likely to be during the initial period of any conventional North Atlantic Treaty Organization (NATO)-Warsaw Pact war.

For the ATB at least, strategic missions like finding and attacking imprecisely located targets such as Soviet rail- or road-mobile ICBMs--which bear both tactical and functional similarities to using U.S. attack submarines against Soviet ballistic missile submarines in bastions--seem to reinforce the idea of single-ship employment that has long dominated Strategic Air Command planning for general nuclear war with the U.S.S.R. Certainly with nuclear weapons, the prospects of sending even two ATBs into the same area after mobile targets would be dim if fratricide is to be avoided. While such fratricide would not be a problem if conventional weapons were used, we might not want to assign multiple ATBs to the same set of targets. At first glance, then, the most plausible ATB mode of employment would evidently be single aircraft operating with great autonomy--which is to say, like nuclear attack submarines.

Future Soviet attempts to counter the ATB might mean that ATB missions would have to be planned and flown so as to take advantage of weather patterns and terrain features in much the same way that submarine skippers take advantage of oceanic thermoclines and ocean-bottom contours to obscure their acoustic signatures. Hence, one long-term result of as-yet-undetermined Soviet countermeasures might be to make successful bomber operations even more dependent than before on the intuitive ability of individual aircrews to cope in real time with complex, rapidly shifting tactical environments.

As in submarine operations, this greater richness and complexity, combined with a large degree of autonomy, would obviously impose greater demands on the tactical skills and judgment of individual aircrews. This kind of cost, however, is one that Americans and West Europeans have historically been far better able to bear than the Soviets. From beginning to end, German experience fighting the Red Army during World War II indicated that the average Soviet soldier possessed "neither the

judgment nor the ability to think independently."[6] Of course, the Germans' experience also confirmed that the Soviet soldier was often willing to sacrifice himself without hesitation in defense of his motherland and, in certain circumstances, to fight with shocking tenacity and brutality.

Have the Soviets made much progress toward overcoming these shortcomings in the years since 1945? In the opinion of many who have studied the Soviet military, there seems little convincing evidence that they have. For example, in the judgment of retired British Brigadier Richard Simpkin, who has followed the U.S.S.R.'s military for several decades, the average Soviet officer serving with troops, up to and including battalion commanders, apparently has only one response to a combat situation: "to play it by the book as far as he can, and then sit back and await new orders."[7] In this same vein, Christopher Donnelly, who heads Soviet studies research at Great Britain's Royal Military Academy (Sandhurst), has concluded that the Soviet soldier, even today, "is not a natural innovator at the tactical level."[8]

Can this assessment of the Soviet soldier be extended to the Soviet airman? Again, there seems no compelling reason not to do so. Consider the performance of Soviet interceptor pilots during the brief portion of the 1969-70 war of attrition between Egypt

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6 Russian Combat Methods in World War II, Department of the Army Pamphlet 20-230, November 1950, p. 3. This document was prepared by a committee of former German officers during the winter of 1947-48. All of them had extensive experience on the eastern front during the years 1941-45; the principal author, for example, commanded in succession a panzer division, corps, panzer army, and an army group.

7 Richard E. Simpkin, Race to the Swift: Thoughts on Twenty-First Century Warfare (Washington D.C. and London: Brassey's Defence Publishers, 1985), p. 52. Simpkin, who retired in 1971, was, for 30 years, an officer of the British Army's Royal Tank Regiment.

8 C. N. Donnelly, "Heirs of Clausewitz: Change and Continuity in the Soviet War Machine," Occasional Paper No. 16, Institute for European Defence and Strategic Studies, 1985, p. 18. Donnelly, however, judges the flexibility of Soviet commanders at the three- and four-star (or operational) level to be another matter altogether (Ibid., p. 17), and German assessments of the Soviet high command during World War II strongly support Donnelly's judgment that higher echelon Soviet commanders may be far less rigid than their subordinates at the tactical level (see, for example, Russian Combat Methods in World War II, pp. 8 and 10-11).

and Israel when they actually fought against Israeli pilots. In the one major Soviet-Israeli air-to-air engagement of this war, eight Israeli F-4s and Mirages engaged at least as many Soviet-flown MIG-21s in a swirling dogfight. But in only a matter of minutes, five of the MIGs were downed with no Israeli losses.[9] Based on firsthand Israeli accounts of this encounter, the crux of the Soviets' difficulties was that, although they went into the fight flying textbook formations and displayed considerable courage, they were unable to react appropriately in the chaotic melee that ensued. Consequently, they committed elementary and often fatal mistakes.

Moreover, while there have been relentless exhortations in the U.S.S.R.'s military press throughout the last decade for Soviet pilots to develop "creative initiative and flexible tactical thinking,"[10] the operative Marxist-Leninist view of pilot initiative and flexibility remains vastly different than that which exists in the West. Here the prototypical situation is the aerial engagement in which the adversary does something unexpected. True, both Westerners and Soviets agree that such situations demand initiative. But for an American or Israeli fighter pilot, the requisite initiative is preeminently understood as a capacity to respond effectively to the unexpected on the basis of finely honed "seat of the pants" savvy; it is something exercised on the spur of the moment in the air. By contrast, the Soviet pilot seems to view initiative in terms of having worked out in advance an appropriate combat variant to counter the adversary's "unexpected" move.[11] To him, initiative is primarily a scientific process that is carried out on the ground. Thus, the broad implication of the Soviets' historic lack of tactical flexibility for the Age of the Stealth seems plain enough. If in fact American efforts to preserve the initial advantages conferred by LOT aircraft do serve to render success in aerial warfare ever more contingent on the initiative and adaptability of individual aircrews, then the human component of this competition, like its technological counterpart, will offer the U.S. and its allies increasing competitive leverage in an area of endemic Soviet weakness. All we need to exploit this weakness more fully is a willingness, where necessary, to put aside old ways of doing business in order to capitalize on the

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9 Born in Battle: Israel's Air Force, Eschel-Dramit, No. 2, 1978, p. 50.

10 Pilot First Class Captain Yu. Priymak, "If One Thinks in an Innovative Manner . . .", Aviatsiya i Kosmonavtika, No. 2, February 1983, p. 33.

11 Colonels Z. Nikitin and Yu. Kislyakov, "Are Models Needed in Tactics?", Aviatsiya i Kosmonavtika, No. 9, September 1983, p.35.

comparative advantages of our people.

This assessment is perhaps most readily substantiated in the realm of air superiority. Empirical tests like ACEVAL (Air Combat Evaluation) and the AMRAAM OUE (Advanced, Medium-Range, Air-to-Air Missile Operational Utility Evaluation) have demonstrated beyond reasonable doubt that in multiple-ship engagements, aircrew situation awareness is statistically the single most important factor in determining outcomes.[12] Inherently, an ATF with substantially lower signatures would give U.S. pilots a built-in situation awareness advantage over Soviet, Eastern European, or other adversaries. Of course, the ability to close within lethal range undetected must be exploited to be of value in combat. Among other things, an inherent edge in situation awareness would "increase the probability of tactical surprise, and allow the side possessing it to control the dynamics of battle."[13] If so, then ATF tactics would very likely evolve toward a strong preference for slashing attacks to attain quick kills against fleeting targets, followed by rapid separation to regain a covert posture. In that case, the individual pilot's ability to adapt more rapidly than the adversary to ever more dynamic and complex tactical situations may, more than ever before, prove to be the narrow margin between victory and defeat in the air.

Suffice it to say, therefore, that the operational security practices, employment concepts, and tactics upon which U.S. airmen have largely relied since the mid 1930s are likely to require substantial revision if the most is to be made of stealthy bombers, cruise missiles, and air superiority fighters in the decade ahead. <u>Weapon systems that lean heavily on stealth to accomplish their missions will demand equally stealthy mindsets if the fundamental advantages of low-observables technology are to be exploited and preserved</u>. Programs to test platform signatures on a regular basis, to monitor operational employment patterns, and to conduct a rigorous search for countermeasures will need to be thought through and in place long before LOT systems begin appearing in operational units. At the same time, the employment concepts and tactics of the Age of Stealth may place increased importance on the skill, cunning, and initiative of individual aircrews, thus playing to one of the West's most enduring areas of competitive advantage. In short, "blue-sky" thinking about stealth suggests that the Air Force's future is likely to be substantially different from its past.

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12 "Man in the Loop Lesson Learned," Briefing, Veda Inc., 1985, Slide 1.

13 Benjamin S. Lambeth, "The Outlook for Tactical Airpower in the Decade Ahead," Rand Corporation Paper P-7260, September 1986, p. 9.

APPENDIX D

CRUISE MISSILES: RESPONSE TO DISCRIMINATE DETERRENCE

Captain E. Fenton Carey, USN

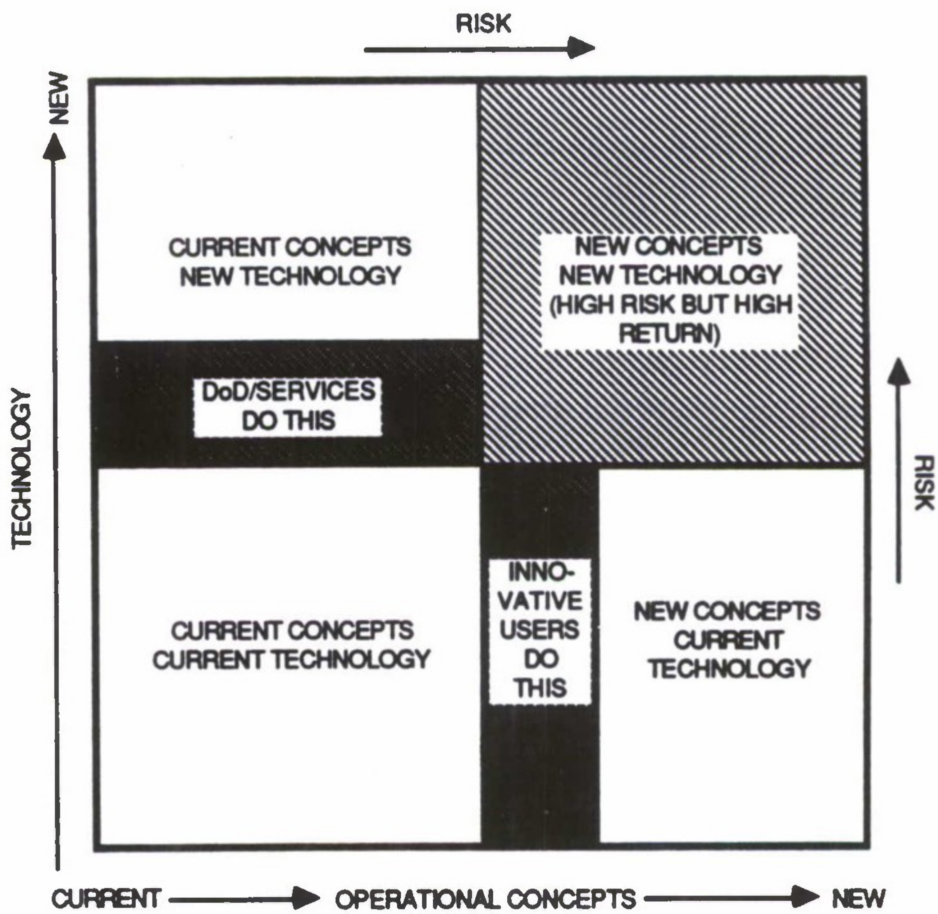
Long-range, extremely accurate cruise missiles can provide the United States with a broader range of strategic and tactical options to apply force discriminately and responsively to deter aggression. Advanced technologies and innovative concepts are ripe for aggressively developing a family of these missiles and employing them against fixed and relocatable targets deep in enemy territory. However, the nation will not realize the benefit of these technologies and concepts unless they are focused and integrated into a total weapon system including the targeting sensors and command, control, communications, and intelligence (C^3I) required to target the missiles. Near-term demonstrations of the technologies and concepts in the early 1990s are key to future development and procurement decisions, and we must be willing to accept high risk if we are to realize their high potential payoff. A new, more capable family of cruise missiles and targeting systems can be built that will improve our competitive advantage over the Soviet Union. But until we can define the new systems, a wide range of demonstrations will complicate any Soviet strategy to counter our next generation of missiles and targeting systems.

DEMONSTRATIONS ARE KEY TO FUTURE TOMAHAWK WEAPON SYSTEM DEVELOPMENT AND PROCUREMENT DECISIONS

Over the last 8 years, the Administration has made a major investment in modernizing our strategic and tactical offensive weapon systems and in replacing our aging ships and aircraft. With new platforms entering the military inventory, the Department of Defense (DoD) and the Services are shifting their attention to optimizing the capabilities of our weapon systems by probing new technologies and new operational concepts, see Figure D-1. However, conservatism in the acquisition process resists major technical and operational innovation and could stymie their ability to make quantum improvements in our current capabilities.

The DoD and the Services must be able to pursue new technologies and tactics across a broad spectrum. If they do not, our competitive advantage will evaporate as our adversaries continue to integrate quickly our technologies into their weapon systems. To fulfill the concepts proposed in the new integrated long-term strategy, the Nation must focus its resources and energy in those areas that will provide the greatest leverage and strength to our ability to respond to aggression with controlled,

FIGURE D-1:
DEMONSTRATIONS OF INNOVATIVE CONCEPTS AND ADVANCED TECHNOLOGIES KEY TO FUTURE PROCUREMENT DECISIONS



discriminate use of force. This includes:

- An increased investment in basic research and advanced development in emerging technologies that capitalizes on our competitive advantage in precision munitions, command and control, and intelligence

- Demonstrations of these technologies with innovative operational concepts that can provide our conventional forces with more selective and more flexible capabilities for destroying military targets deep in hostile territory.

The recommended demonstrations for cruise missiles fall into three basic categories: (1) new concepts with current technology, (2) current concepts with new technologies, and (3) new concepts with new technologies, see Figure D-2. They are phased to take advantage of current capabilities--the Tomahawk weapon system (TWS)--and to allow technologies to mature, see Figure D-3. This will enable us to capitalize on the large investments already made in existing platforms and systems and provide a basis for comparing the new concepts and technologies with current operational capabilities.

The current TWS consists of a family of sea-launched cruise missiles (SLCMs) which includes: the Tomahawk antiship missile (TASM) and the conventional and nuclear Tomahawk land-attack missiles (TLAMs); weapon control systems on Tomahawk capable surface and submarine platforms; and shore-based Theater Mission Planning Centers (TMPCs), see Figure D-4. The Navy is outfitting 200 naval surface and submarine platforms with a Tomahawk capability, acquiring approximately 4,000 SLCMs and upgrading the shore-based TMPCs to increase mission planning responsiveness and throughput.

SENSOR-TO-SHOOTER TARGETING CONCEPT USING CURRENT TECHNOLOGY--NEW CONCEPTS/CURRENT TECHNOLOGIES

The first category would capitalize on the large investment in the TWS to demonstrate a sensor-to-shooter targeting concept using a Tomahawk-capable surface ship, see Figure D-5. An essential ingredient to effective employment of smart weapons that combine high accuracy and long range and that are responsive in wartime is timely information on the threat environment and the targets. As depicted in Figure D-6, approximately 90 percent of the time to plan a conventional TLAM mission today is spent collecting source materials and developing critical mapping, charting, and geodesy (MC&G) data bases. Once collected, mission planning and data distribution consume the bulk of the remaining time needed to employ the missile. In wartime, the limited availability of source material and of communications to

**FIGURE D-2:
RECOMMENDED DEMONSTRATIONS OF INNOVATIVE CONCEPTS AND ADVANCED TECHNOLOGIES**

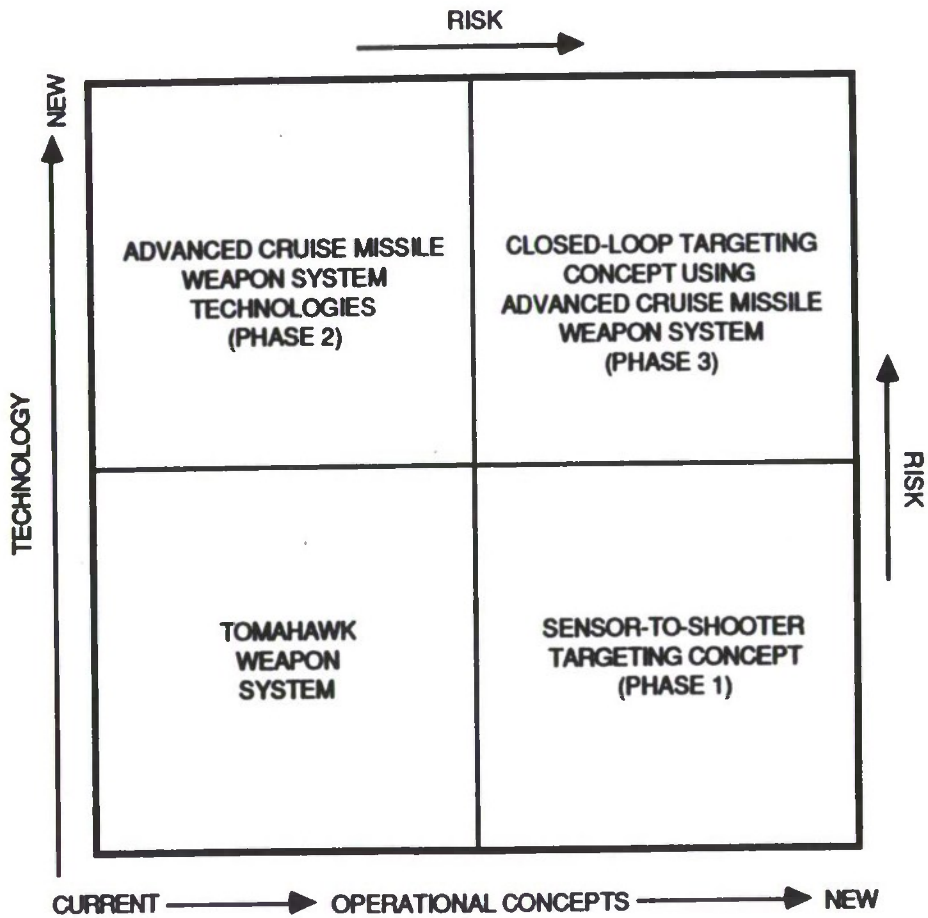


FIGURE D-3:
DEMONSTRATION PLAN

- **PHASE 1: SENSOR-TO-SHOOTER TARGETING CONCEPT USING CURRENT TECHNOLOGY - 1991**
- **PHASE 2: ADVANCED CRUISE MISSILE WEAPON SYSTEM TECHNOLOGIES - 1992/93**
- **PHASE 3: CLOSED-LOOP TARGETING CONCEPT USING ADVANCED CRUISE MISSILE WEAPON SYSTEM TECHNOLOGIES - 1993/94**

Figure D-4
Tomahawk Integrated Weapon System
"Except for Sensors and C^2"

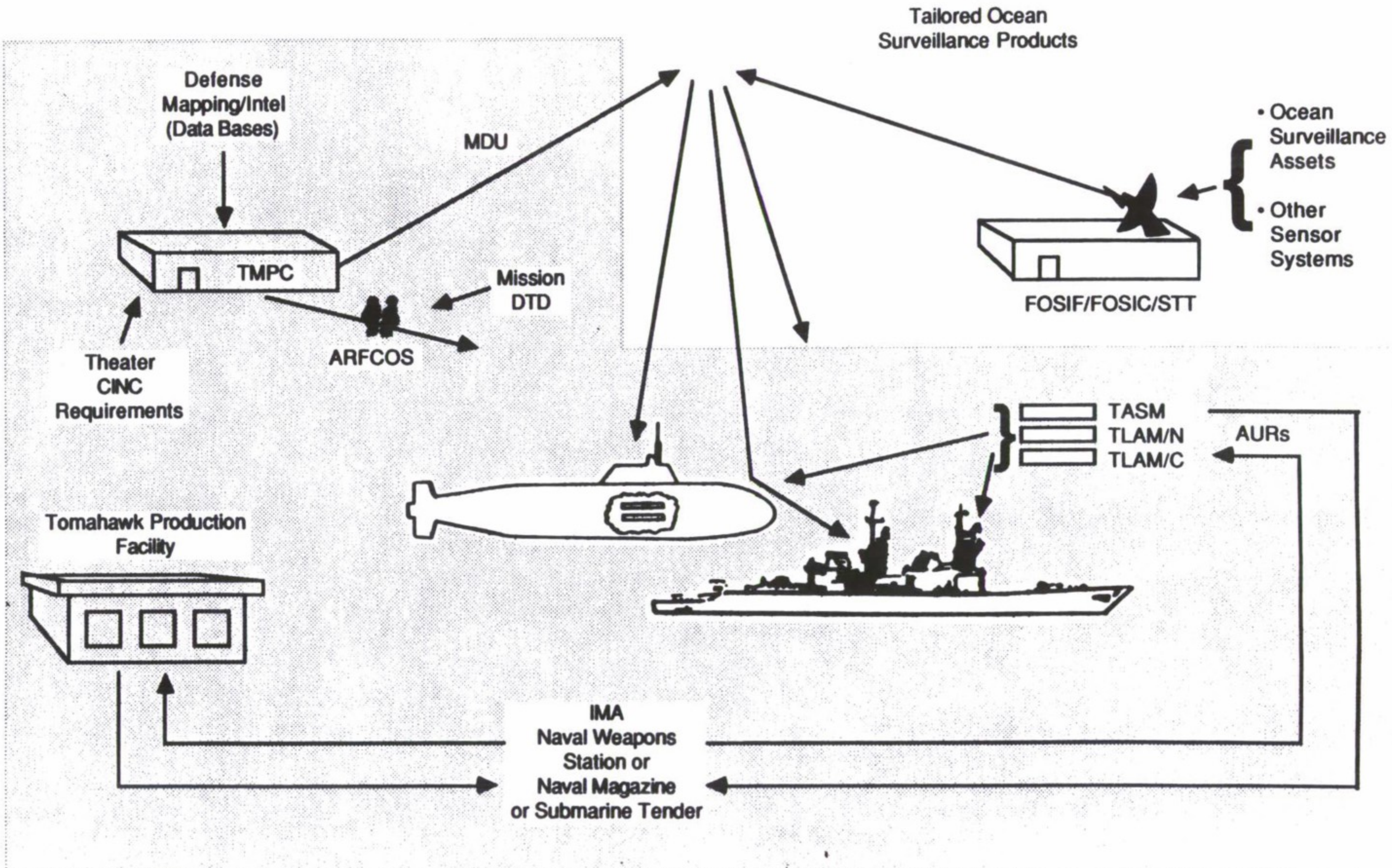


FIGURE D-5:
SENSOR-TO-SHOOTER TARGETING CONCEPT

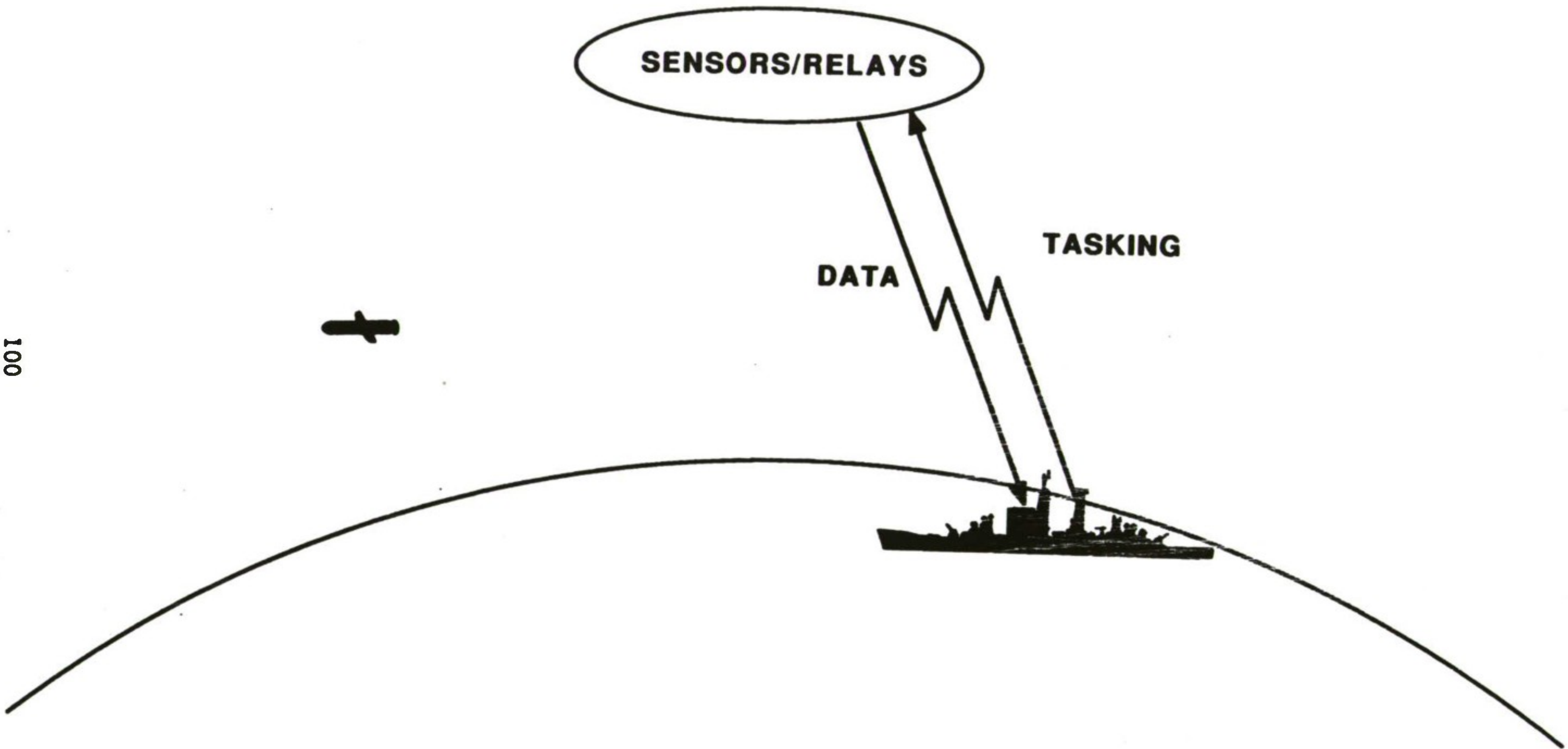
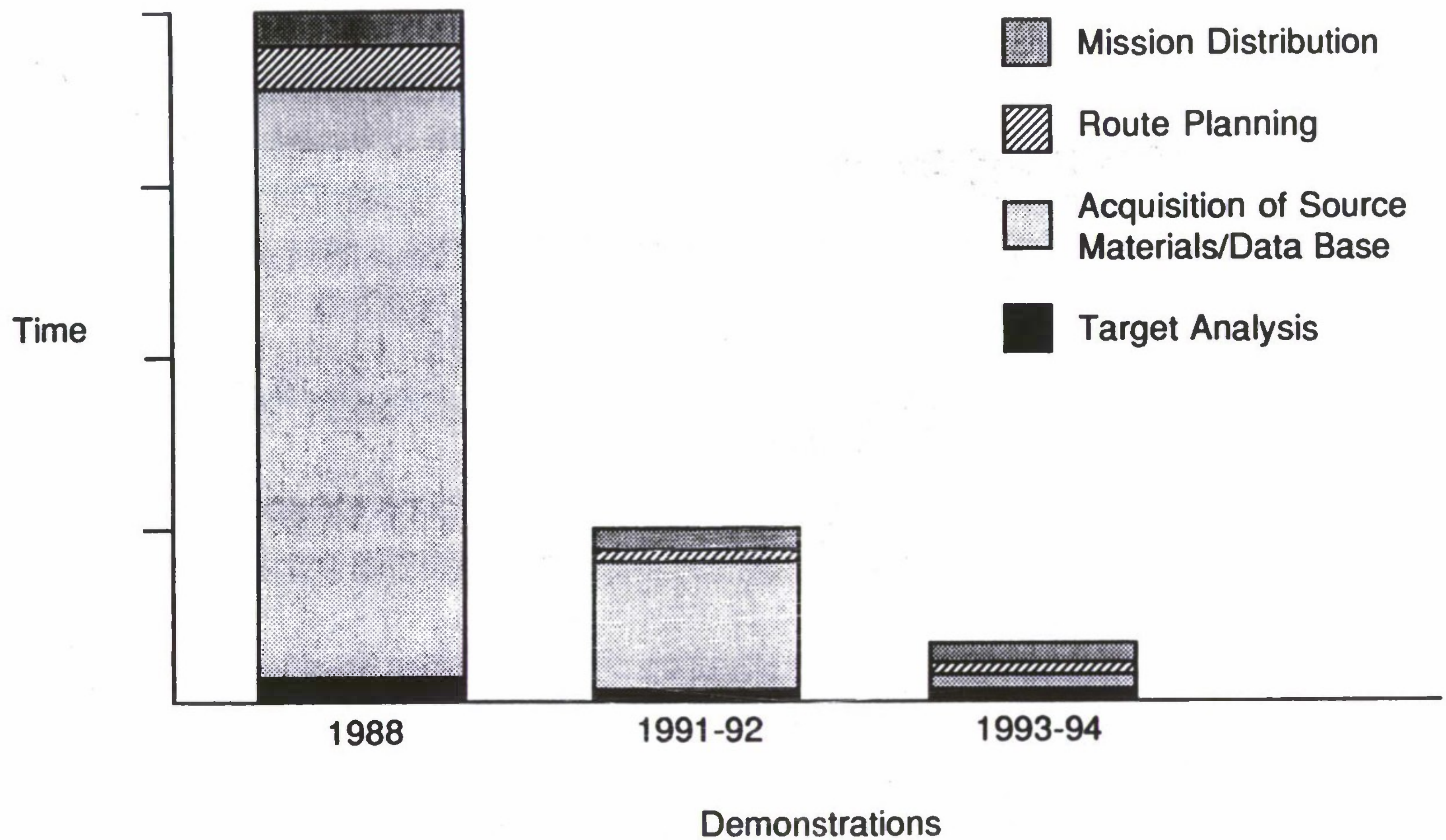


Figure D-6:
Conventional TLAM Responsiveness



distribute the data to Tomahawk platforms will further limit TLAM responsiveness and suggests that there is a need for a more direct link between the sensors and launch platforms and for a mission planning capability on those platforms.

The Navy is developing the latter capability and will deploy an engineering development model of a TLAM planning system afloat in a Tomahawk platform in 1991. The afloat planning system (APS) will migrate the software of the Congressionally approved Theater Mission Planning Center Upgrade program to down-sized computer hardware afloat. This will distribute the planning capability for conventional TLAM across the fleet, improving the weapon system's responsiveness and survivability. Although the APS will greatly reduce the time required to develope critical MC&G data bases, acquiring source material will still consume a large percentage of the time and in wartime may not be practical unless obtained organic to the battle group.

Current technology is available to design smaller, lower cost space systems and medium-range remotely piloted vehicles (RPVs), which could provide battle groups with an organic capability: to launch communications and sensor payloads, to task them directly, and to receive data directly from them to support cruise missile mission planning and targeting. The sensors need not last for many years nor have the capacity of peacetime systems but must be replaceable in wartime and provide information on small areas of interest to the operational users. Direct connectivity from the space systems or RPVs to the Tomahawk platforms could reduce the time required to gather source material by as much as 98 percent and would provide an organic sensor-to-shooter capability for the Battle-Group Commander.

The Working Group proposes that the Navy demonstrate by 1991 the sensor-to-shooter concept in conjunction with developmental tests of other Tomahawk weapon system pre-planned product improvements, see Figure D-7. The test could demonstrate:

- A medium-range missile with the current circular error probable (CEP); this would use the current Tomahawk missile with extended range, and global positioning system (GPS) and new Digital Scene Matching Area Correlator (DSMAC) IIA system for navigational update

- A near-real-time mission planning capability on surface ships using real-time sensor data and the afloat planning system

FIGURE D-7:
SENSOR-TO-SHOOTER TARGETING
CONCEPT DEMONSTRATION
1991

- MEDIUM RANGE MISSILE WITH CURRENT TLAM CEP
- NEAR-REAL-TIME MISSION PLANNING ON SURFACE SHIPS
- DIRECT SENSOR-TO-SHIP CONNECTIVITY FOR MISSION AND TARGETING DATA

- Direct sensor-to-shooter connectivity for linking critical mission-planning and targeting data directly to a Tomahawk platform using RPVs and space systems developed jointly with the Defense Advanced Research Projects Agency's (DARPA's) Advanced Satellite Technology Program (ASTP) or other similar space-system/launcher development programs

ADVANCED CRUISE MISSILES TECHNOLOGIES--CURRENT CONCEPTS/NEW TECHNOLOGIES

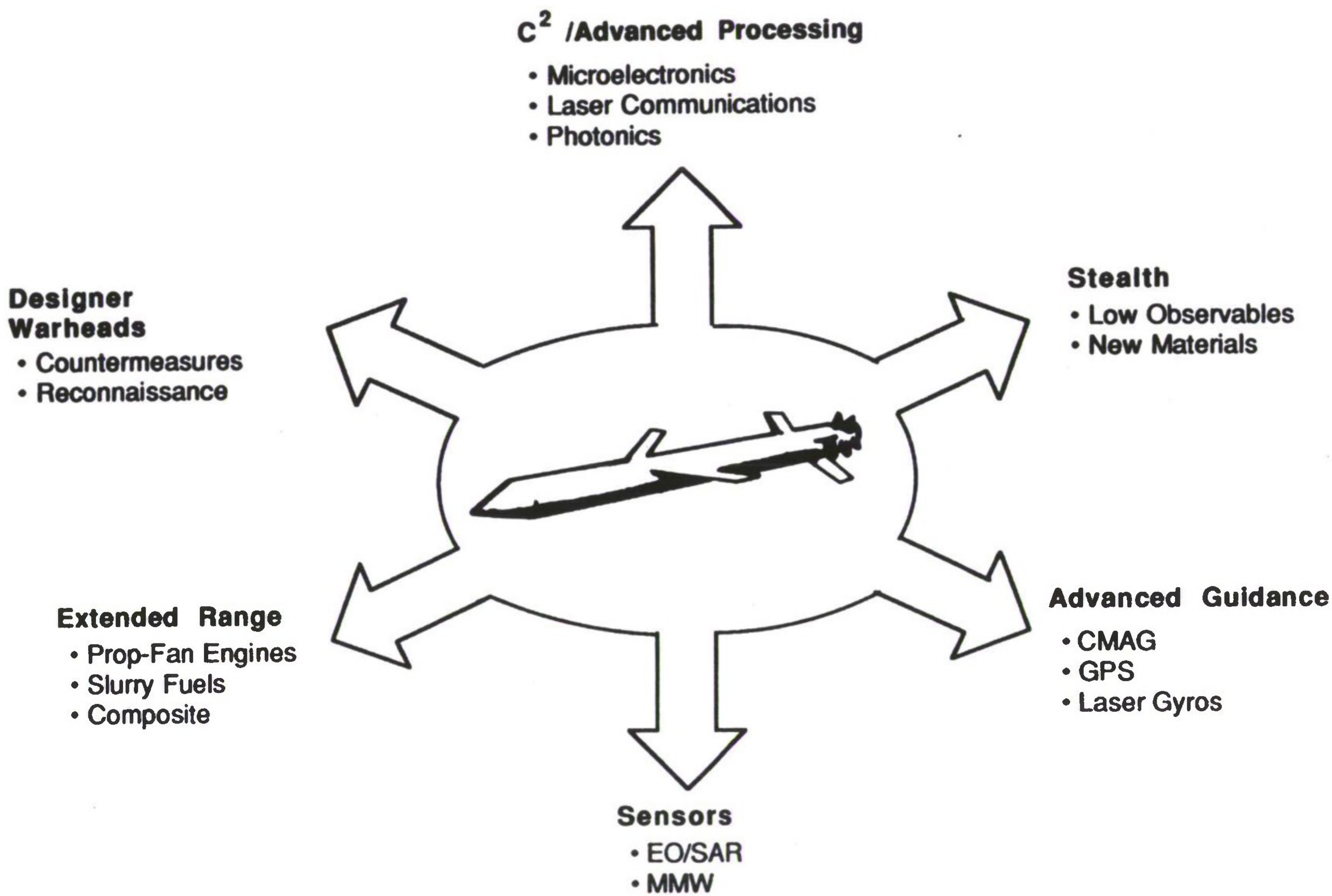
A second set of demonstrations would exploit the explosion in new technologies and manufacturing techniques to press the physical limits in areas such as missile technology, advanced guidance, advanced processing, mission planning, command and control, and sensors as depicted in Figure D-8. These technologies could provide quantum improvements in all elements of a future cruise missile weapon system.

- Missile. Initial efforts could focus on fostering technologies that would enhance missile range, accuracy, autonomy, lethality, and survivability.

  -- **Range.** Prop-fan engines with high bypass using propulsion computational fluid dynamics (CFD), and improved gas turbine ceramic components could improve specific fuel consumption by as much as 50 percent. A lighter missile made from composites or alloys could increase fuel capacity by as much as 15 to 25 percent. Combining these technologies with computer-aided design/computer-aided manufacturing (CAD/CAM) techniques to improve flight-vehicle design could reduce engineering costs, and with high specific- heat slurry fuels could triple the current missile range.

  -- **Accuracy/Autonomy.** New navigation and guidance techniques could provide an order-of-magnitude improvement in weapon-system delivery accuracy. Ring-laser, fiber-optic gyros combined with a laser or millimeter wave sensor could enable the missile to navigate and avoid obstacles autonomously to the target. Embedded microprocessors could provide real-time aerodynamic feedback to the missile enabling it to home on the designated target with great precision.

Figure D-8:
Advanced Cruise Missile Weapon-System Technologies



-- **Lethality.** Although accuracy usually buys more in an effective trade-off with the warhead, new warhead packaging and penetration techniques, higher density explosives, and more diverse payloads could provide the flexibility needed against a wider range of targets.

-- **Survivability.** Key to increasing missile survivability are new composite materials, manufacturing techniques, airframe shapes, passive sensors, which could greatly reduce observables and integrated sensors and microprocessors that could enhance missile autonomy. New high-performance engines can provide missiles with a great range of speeds and quicker responsiveness to react to the threat environment.

- **<u>Weapon Management Systems (Platforms and Interfaces)</u>.** Technologies in this area must focus on improving the ability of current Tomahawk platforms to plan and launch missiles quickly and to launch various missile configurations.

-- **Planning Responsiveness.** Emerging computer, artificial intelligence, flight simulation, storage, and display technologies could permit mission planning in real time by embedding the capability in every aspect of the strike planning process. Given mission objectives, expert systems could automatically apply doctrine and geophysical, threat, and other tactical constraints to provide the decision maker with employment options that could optimize missile performance, accuracy, and survivability. Advanced missile simulators could then automatically validate missions prior to their use.

-- **Launcher Configurations.** New missile shapes and launch modes will require new encapsulation techniques to permit the use of existing ship and submarine interfaces and to develop new ones for aircraft.

- **<u>Command and Control</u>.** Of all of the objectives for a future cruise missile--range, accuracy, survivability, responsiveness--responsiveness is the greatest challenge. Our ability to meet this challenge depends

on how well we can capitalize on the evolving technologies in supercomputers, microcomputers/processors, and communications to process and distribute data.

-- **Advanced Computers/Processors.** Galium arsenide (GaAs) microprocessors could provide the building block for parallel processors. Touted as the semiconductor of the future, GaAs has inherent radiation hardness and ability to handle switching frequencies 5 times as high as silicon. In the long term, optical computers have the potential for high speed, parallel operations and dense interconnections and could revolutionize our ability to process immense quantities of data quickly. In the near-term, there are special purpose applications of the technology such as for optical storage to hold billions of bytes of data. Erasable optical storage techniques are maturing and breakthroughs in reprogrammable optical storage for platform or missile applications appear imminent.

-- **Communications.** New thrusts in high-power, solid-state lasers could provide more robust and capable communications among platforms and between sensors and platforms. This includes the capability to communicate directly with submerged submarines.

- <u>Sensors</u>. New sensor technologies are also ripe for exploitation and could provide active or passive surveillance, detection, and discrimination day and night and in adverse weather using synthetic aperture or submillimeter wave radar techniques. Possibly the biggest gain in sensor technology is miniaturization. Miniaturization will permit the use of sensors in the missile, in RPVs and in space-based systems that can operate autonomously and link their data directly to cruise missile platforms.

The Working Group proposes that DoD and the Services conduct a series of demonstrations during 1992 and 1993, see Figure D-9, to prove the feasibility of:

- Doubling the current TLAM range and achieving near-zero CEP; the missile would also autonomously navigate and avoid obstacles and enemy defenses enroute to the target

- Planning cruise missile missions in near-real time in submarines and aircraft

FIGURE D-9:
ADVANCED CRUISE MISSILE WEAPON-SYSTEM
TECHNOLOGY DEMONSTRATION
1992-1993

- LONG-RANGE MISSILE WITH NEAR-ZERO CEP
- AUTONOMOUS MISSILE TERRAIN FOLLOWING AND THREAT/OBSTACLE AVOIDANCE
- NEAR-REAL-TIME MISSION PLANNING ON SUBMARINES/AIRCRAF
- DIRECT SHIP-TO-MISSILE AND MISSILE-TO-SHIP CONNECTIVITY FOR TARGETING/RETARGETING
- SATELLITE SENSOR/RELAY CONNECTIVITY WITH SUBMERGED SUBMARINE
- MMW SENSORS FOR RETARGETING CAPABILITY
- LAUNCH OF SENSORS FROM MOBILE PLATFORMS

- Providing direct connectivity between targeting sensors and cruise missile platforms, between surface platforms and submerged submarines, and between surface platforms and the missile for retargeting

- Launching advanced space sensors from mobile platforms to provide real-time targeting quality directly to cruise missile platforms.

CLOSED-LOOP TARGETING CONCEPT USING ADVANCED TECHNOLOGIES--NEW CONCEPTS/NEW TECHNOLOGIES

. Melding the new concepts and new technologies into demonstrations of an integrated weapon system will be the biggest challenge but could provide the greatest payoff. Though risky, these demonstrations could provide the breakthroughs needed to widen our competitive advantage over the Soviets and to guide future procurement decisions to support the new long-term strategy.

Basic research and advanced development should commence immediately in those high-risk areas needed to demonstrate a closed-loop targeting capability, see Figure D-10. Since an advanced deep-interdiction SLCM could transit vast expanses of enemy territory for several hours, the launch platform will need the capability to assess continually the target area and have the capability to retarget the missile in flight. Such a closed-loop weapon system could include:

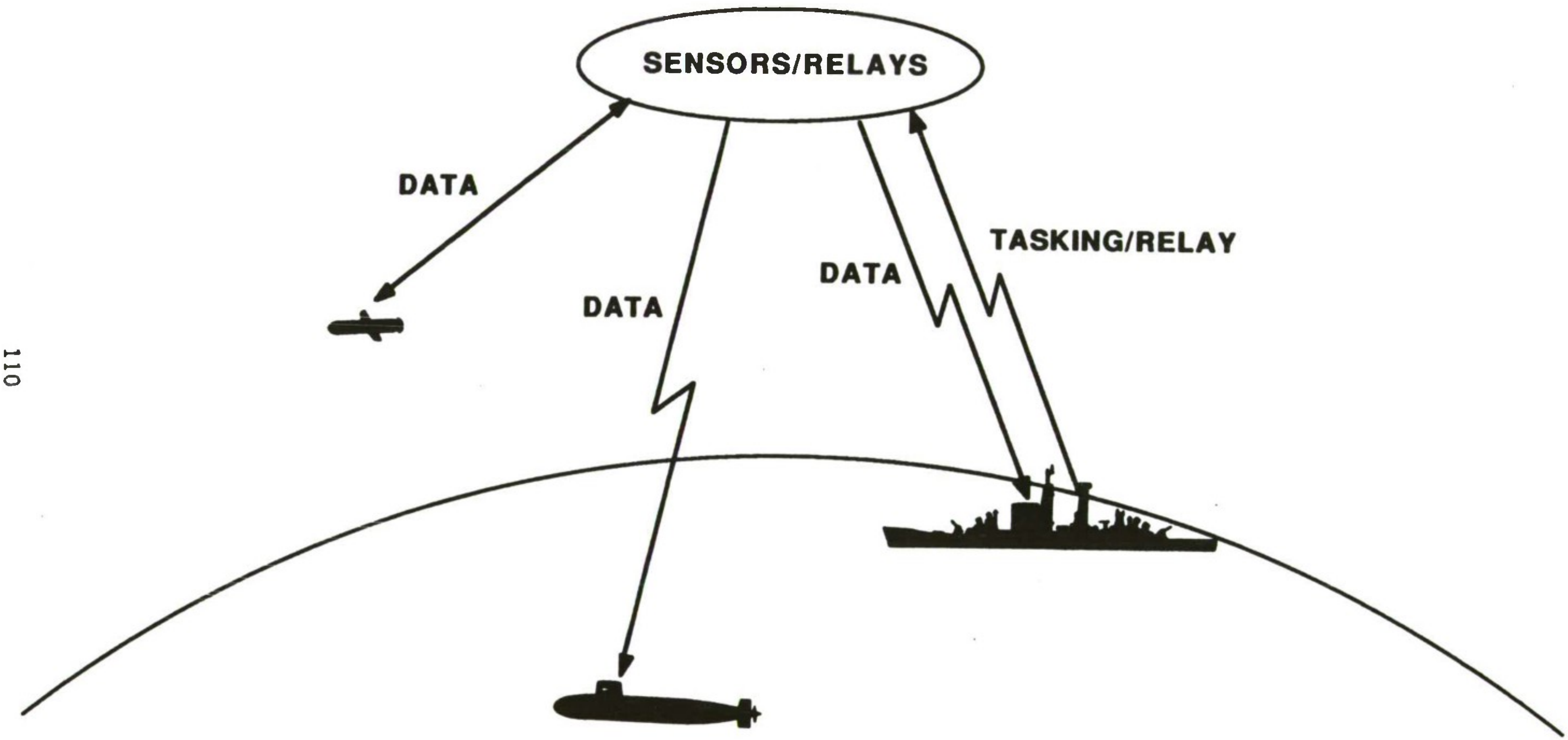
- Sensors to detect and discriminate fixed and relocatable targets and directly link their information to cruise missile platforms

- Planning systems on the platforms capable of planning missions and launching missiles in near-real time

- Low-observable missiles, which are highly accurate, capable of flying long ranges, and reprogrammable in flight.

To enhance the wartime utility of the system, survivability and sustainability is paramount and must be built into all of its elements. This could include submarines that can release their missiles covertly and activate them remotely, sensors that are launchable from mobile platforms, and missiles that can autonomously avoid or attack hostile defenses.

FIGURE D-10:
CLOSED-LOOP TARGETING CONCEPT



The Working Group proposes that DoD in close coordination with the Unified and Specified Commands conduct a series of demonstrations during 1993 and 1994 to bring the advanced technologies and innovative concepts together as a weapon system, see Figure D-11. Objectives of such demonstrations could include:

- Launching an extended range missile from a surface ship and attaining the current TLAM CEP against a relocatable target; the ship could request that a mobile platform launch a space sensor on demand to provide it with real-time targeting/retargeting data; the space sensor could link the data directly to the ship, which would plan the mission in real time and launch the missile; the desired total elapsed time from space-system launch to missile launch is less than a day

- Launching a long-range missile from a submerged submarine and attaining a near-zero CEP against a fixed target; the submarine could receive mission planning and target data directly via relay while at operational depth and speed, plan the mission, and launch in near-real time

- The same basic objectives as the first demonstration with the additional requirement that from request for space-sensor launch until the ship receives the data would be near-real time, and the missile would be launched and retargeted in flight against a relocatable target.

FIGURE D-11:
CLOSED-LOOP TARGETING CONCEPT AND ADVANCED
CRUISE MISSILE WEAPON-SYSTEM TECHNOLOGY
DEMONSTRATION
1993-1994

- LOW-OBSERVABLE, EXTENDED-RANGE MISSILE WITH NEAR-ZERO CEP
- LAUNCH-ON-DEMAND OF "ORGANIC" SPACE SENSORS FROM MOBILE PLATFORM FOR TARGETING/RETARGETING
- REAL-TIME MISSION PLANNING ON SURFACE SHIPS
- FORWARD PASS FROM SHIP-TO-MISSILE WITH MISSILE-TO-SHIP CONNECTIVITY FOR REAL-TIME RETARGETING
- DIRECT SENSOR-TO-SUBMARINE/AIRCRAFT CONNECTIVITY FOR MISSION PLANNING AND TARGETING DATA

APPENDIX E

SUPPORTING PRECISION, DEEP-STRIKE MISSILES

Richard Brody

The technology to support deep, conventional cruise missile strikes with circular error probables (CEPs) as small as 1 to 3 meters is potentially available. A coherent program to develop it is still needed. Two prerequisites though are a much more fine-grained analysis of target vulnerability/munitions effectiveness appropriate to such accuracies and an equal consideration of the non-accuracy aspects of alternate precision guidance systems including reliability, robustness, flexibility, as well as the costs, and survivability of necessary outside supporting C^3I.

One of the major changes in weaponry over the next 20 years will be a rise in the importance of deep, conventional strikes, deep into the homelands of the major adversaries, in future major East/West conflict. Of course, such deep strikes were a major feature of World War II, most particularly the Allied bombing campaigns against Germany and Japan. The B-29, in particular, represented a means to deliver conventional strikes to well over a thousand miles. Several factors led to a downgrading of priority for deep, conventional strikes after World War II, however. Improving air defenses made repeated deep conventional sorties increasingly expensive. Surveys after the war brought into question the real value of such strikes. On the other hand, nuclear weapons seemed a far more attractive alternative deep strike means in a period when any major U.S./Soviet conflict was seen as almost certainly being a nuclear one.

The revival of interest in deep, conventional strikes derives not surprisingly from a reversal of those trends. With the growth of secure nuclear capabilities on both sides, the incentives to avoid nuclear escalation are becoming very obvious, even in the face of some number of conventional strikes on the homeland (escalation to the nuclear level promises only to make things worse). On the other hand, the growth of precision, range-independent accuracy is chipping at both sides of the cost-effectiveness reasoning for abandoning deep, conventional strikes. Greater accuracy means a few conventional bombs delivered right on the target <u>may</u> have the same effect as either a massive raid with inaccurate weapons or a few weapons of mass destruction. Range-independent accuracy allows the use of standoff missiles instead of manned aircraft for penetration--the cost of delivering attack by missiles is much less sensitive to air defense effectiveness than the cost of using aircraft for repeated raids. Finally, stealth may offer a return of repeated strikes at low attrition rates by penetrating manned platforms.

BACKGROUND ON RANGE-INDEPENDENT ACCURACY

The technology to support range-independent precision accuracy, down to 1-3 meters CEP is now at hand. This does not mean that it has arrived or that it inevitably will arrive--one can make the argument that it could have been available now given even a moderately high-priority coherent program over the past decade. However, at this point there can be little doubt about the technical possibility of such extreme accuracy. Indeed, there are a variety of options for missile guidance that meet or approach this goal involving various combinations of radar Terrain Comparison (TERCOM), optical Digital Scene Matching Area Correlation (DSMAC), Global Positioning System (GPS), infrared (IR) laser Cruise Missile Advanced Guidance (CMAG), passive IR matchers, and so on.

Of particular interest at present are the advantages and costs of DSMAC, improved DSMAC, CMAG, and GPS for future long-range conventional cruise missiles. In addition to using various sensors and algorithms, they differ in precision and cost, as well as in other characteristics. These other characteristics, such as reliability and flexibility, may be the most important in distinguishing the various alternatives. Moreover, in considering these other characteristics, as well as the variables of precision and cost most commonly focused upon, the necessary guidance support system outside the hardware carried in the missile will be as or more important than the actual machinery that goes in each missile.

First, it is worth briefly reviewing the theory of why one wants accuracy. The traditional physics statement, is that accuracy is worth the cube of yield. A tenfold increase in accuracy would allow the delivery of a thousand times less explosion for equal effect (and a hundred times better accuracy would allow a millionfold reduction in yield). The practical epitome of this sort of analysis is involved in Strategic Defense Initiative (SDI) concepts for "smart rocks." These allow replacing Spartan large-yield nuclear warheads for exo-atmospheric Antiballistic Missile (ABM) with a light "hit-to-kill" kinetic energy interceptor, which need not explode at all.

However, this example illustrates the limits of this simplistic approach to the value of higher accuracy. On the one hand, once accuracy is adequate to hit an opposing remote vehicle (RV), further improvements in accuracy (to hit a precise spot on the RV) are likely to have much lower return. On the other hand, the high yield of Spartan not only made up for inherent guidance inaccuracy, but also provided some insurance against target location uncertainty due to chaff clouds or other forms of local decoys. Once CEP becomes much smaller than target area

(including the effects of target location uncertainty), there is very limited payoff to higher accuracy. At accuracies below a few meters, relatively few targets show obvious payoffs to further improvements (with the notable exceptions of reentry vehicles and bridge piers).

BETTER CONSIDERATION OF THE VALUE OF ACCURACY

The simplest and most common way of estimating the value of improved accuracy is to compare the number of missiles required to get a given damage expectancy against a target versus the cost of gaining that additional accuracy. At a minimum in this calculation, the effects of accuracy must be considered not only for reducing numbers of missiles required, but also for economizing on the number of launch vehicles to fire those missiles as well as for reducing the logistics train needed to support those launchers.

A more basic problem derives from trying to measure the value of a new capability using old analytical tools. A neat symmetry is that super high accuracy does little good against a large, uniform target and that with only moderate accuracy, there is little pay-off to doing a very fine-grained target analysis. Despite the extremely good resolution of overhead photographs, such resolution is used mainly for technical intelligence. Beyond only partially adaptable saboteurs manuals, there has been little work on the exact <u>identification</u> as well as location of the exact points in targets that are vulnerable to a conventional strike. More typically, vulnerable area in a building or vehicle is not treated as specific lucrative locations to aim at but as inputs to the probability of kill given a hit on the target as a whole. Small, hard subtargets (such as fuel hydrants, and so on) may not have been considered in targeting at all.

In large part, such simplifications made sense because the accuracies were not available that would make a more fine-grained analysis worthwhile. Now that they are, it is necessary to reexamine munitions effects calculations against a broad class of plausible targets. At this point, such analysis is virtually a precondition of an informed decision for putting resources into much better accuracy.

The general point, of course, is that the more one uses accuracy to substitute for explosive power, the more one had better have chosen the right point. In the case of a point target, or a target subject to one-point failure at a critical node, there need be no ambiguities. Against an area target, such as a runway or deployed infantry unit, there may be no <u>single</u> place that, if hit accurately, will cause target failure. Most complex targets, whether factories or ships or air bases, will fall between these extremes. Some points will be more important

than others; however, destroying only a few points will not be the same as destroying the target as a whole. Rather, certain functions will be interrupted to certain extents for certain periods. The value attached to such interruptions may not be obvious and can be expected to vary with specific circumstances: one may wish to shut down operations for a time, or to keep the enemy from using certain facilities (e.g., missile assembly at an SNA base), or to suppress defenses momentarily to clear the way for a follow-on attack. Determining the best points for precision attacks as well as the value of such strikes requires knowledge not only of the fine structure of enemy operations, but also of how the enemy's operations interact with our campaign plans.

A special variation of the importance of knowing more about the targets to determine the value of higher accuracy is the need to know more about the "antitarget", objects (such as civilians or our own troops) near the target that should not be harmed. Such collateral damage will be a function of CEP in determining (1) the chance that an antitarget offset from the aimpoint will be hit by stray shots and (2) how many (and how large) shots must be fired in the first instance to gain adequate coverage of the primary target. In the latter regard, CEP reductions ease both portions of the dual criterion simultaneously (obtaining a desired military effect while minimizing collateral damage) up to the point that CEP reductions are helpful in reducing shots required (i.e., until CEP is small compared to target area). Beyond that, CEP, as usually defined, may well be of much more secondary importance. If a few antitargets are scattered inside a target area (e.g., a prison inside a headquarters), accuracy may allow avoiding those specific points. However, with a highly accurate weapon, collateral damage will more commonly come from gross errors, such as guidance failure or mismatch. In that case, improving the reliability and the failure/dud recognition abilities of missiles may well do more to reduce collateral damage than improvements to nominal CEP.

IMPROVEMENTS TO GUIDANCE OTHER THAN ACCURACY

Similarly, CEP alone may be an inadequate means for comparing alternate guidance systems for military effect. Particularly where CEP is very low, systems reliability may be a more important determinant of weapons requirements than improved accuracy. This consideration already applies to many nuclear strike missions. At least until results come through from the more fine-grained targeting analysis discussed above, concerns for reliability and robustness are likely to be of more importance in considering the desirability of alternatives to DSMAC (e.g., improved DSMAC, GPS all the way, or CMAG), than CEP by itself.

It can be difficult to quantify comparisons among the reliability and robustness of alternate guidance systems. They will vary in different ways with target and season as well as with specific enemy countermeasures. Moreover, with guidance systems such as TERCOM, DSMAC, and CMAG, which use variations on map matching, there will be a trade-off between the acceptable reliability of match and the number of available alternative update points (places with sufficient contrast to produce the match reliability). A relatively poor system would have a large set of targets for which there was no satisfactory nearby update point. Almost as bad may be having only one or a few satisfactory alternatives, since the enemy will often be able to do nearly as good as job at predicting them as we will.

Another key feature of alternative guidance systems is the ease and rapidity with which new targets can be incorporated into guidance packages. The current system is far from satisfactory even for handling a few unplanned for targets at the national level in a crisis. Ideally, theater users should be able to rapidly plan a strike against a "pop-up" target. In addition, peacetime costs need to be minimized for building up a library for predictable targets. Obviously, these two approaches interact with each other: one can checkerboard large areas with preplanned update points to minimize the additional work required for generating a specific path to any particular target. This approach, however, puts the highest demand on efficient peacetime generation of a large library of alternatives. It also demands an efficient and rapid means to transfer the necessary targeting information from the library to the missiles' fire control. Hand delivery of tapes is the most primitive alternative. High-speed data transmission should be faster, but may have to be very high capacity as well as survivable. On the other hand, optical disk technology should make it practical to proliferate global or at least theater libraries for attacks on preplanned targets directly to the users.

In terms of robustness, reliability, and rapidity of dealing with new targets, GPS is quite different from the map-matching schemes. Beyond identifying target locations within the GPS coordinate system, a matter easily passed through data communications, no further information is required for route planning (though, of course, information on intermediate terrain and defenses may be valuable for increasing penetration probability). Since a GPS receiver can be made very light compared to most other guidance systems, there is a large incentive to add it on for at least mid-course guidance--depending on the target, DSMAC or CMAG terminal update may or may not be valuable. However, for targets in the Warsaw Pact countries. Jamming of GPS satellites may be a major problem as may be more general issues of GPS satellites' survivability. This leads, however, to the next section.

GUIDANCE EXTERNALS' COSTS AND CHARACTERISTICS

Much analyses on the costs and characteristics of guidance systems focus on the hardware that goes in the missile. For most range-independent precision guidance schemes, this will be only a fraction of the system's true costs. As just noted, a GPS-guided system requires real-time communications with GPS satellites at least intermittently throughout its flight. While it is true that we will be buying GPS satellites anyway and that it costs little to add an additional user, the more we use GPS for vital wartime missions, the more cost-effective it will be for the Soviets to target the satellites for hard or soft kill. If adding additional users to GPS makes it a more worthwhile target and hence increases requirements for GPS defense (proliferation, maneuvering, shootback), the costs of those defenses may be a part of the true cost of a GPS guidance scheme.

Map-matching guidance systems also depend on components external to the missiles. They require a reconnaissance system (often national reconnaissance systems) to acquire maps into the generally different perspective of missile sensors (often differing from the reconnaissance in time, look angle, or wavelength) and to translate those update scenes into usable planned mission tapes for the missiles. As noted, there will be trade-offs between real-time preparations during a conflict against actual targets <u>versus</u> peacetime prior preparations against the range of possible targets. Issues of survivability of these systems as well as the timeliness of these alternatives will be important. The cost of making a block change to a new guidance scheme may include the necessity of rebuilding a library of preplanned update scenes.

FIXED VERSUS RELOCATABLE TARGETS

The discussion to this point has focused on guiding a missile to a point on a map and hoping that the target will be at that point. This introduces a potential source of error in that the target may not be precisely located. If a target is fixed and visible, this error should be fairly small. However, the error will generally be larger, if the target is located by a global reference system, such as GPS, rather than merely relative to local features, such as by DSMAC. On the other hand, if the target moves, the error could be arbitrarily large.

Obviously, this scheme of trying to hit something by aiming at a point on a map works least well for a constantly and unpredictably moving target, for example a ship at sea or an aircraft in flight. Many ground targets, however, move only occasionally. Combined with near-real-time reconnaissance and rapid conversion of a location into a flight plan, these can be

treated as temporarily fixed targets. Such relocatable targets as missile launchers, command centers, and radars are often soft area targets against which a good cluster munition may be more useful than extreme accuracy. Moreover, forcing these facilities to increase their movement frequency, so as to dodge this sort of attack, may be the equivalent of at least a partial soft kill in terms of reduced effectiveness.

Even over the next 20 years, development of the capability to do on-board automatic target recognition for direct attack against a mobile target, including discrimination against deliberate as well as natural decoys, is far from certain. It should not hold up the deployment of the point-on-a-map targeting system discussed. On-board target identification may prove most feasible for distinguishing valuable targets in a very confined area (making up for any local rearrangements). However, for the indefinite future, strike reconnaissance may well require a man in the loop and probably a manned aircraft. Radar harassment drones are an important exception.

OTHER TECHNOLOGIES CRITICAL TO PRECISION DEEP STRIKE

The bulk of this appendix addresses guidance support for precise, conventional deep strike missiles. Obviously, several other aspects of such missiles are important, most notably their range/payload capability, launching platforms, delivery trajectory, and penetration probability. While all of these are important, they seem either subject to relatively moderate changes over the time period considered or, like penetration probability and munitions options, worthy of separate discussion.

One comment worth making concerns the desirability of a long-range, conventional air-launched cruise missile (ALCM) as well as sea-launched cruise missile (SLCM). Air launch may have major advantages in terms of missile flexibility (including warheads tailored to the specific targets), rapid deployment, and sustained delivery rate. With fairly marginal changes, the sea-launced Tomahawk Land-Attack Missile-C (TLAM-C) could be adapted for air launch (indeed the basic Tomahawk was originally designed with that option in mind). In addition, the deployment by the Air Force of a follow-on to the ALCM-B suggests a further opportunity for developing a conventional as well as a nuclear ALCM capability for SAC, either by taking over ALCM-B capability retiring from the nuclear mission or incorporating conventional capability into follow-on systems. The choice here is not obvious, nor is it obvious that these are exclusive alternatives.

A parallel option would be to improve provision for at-sea reloadability of SLCM launch ships. This could serve both to support sustained strikes and to avoid having to choose before sailing the exact mix of missile types carried aboard combatants.

Greater emphasis on reloadability may be easier for launchers that do not also support air defense missiles because surge launch rate may be a less overriding priority.

Two considerations should be kept in mind, however, in the design of a follow-on conventional ALCM and SLCM. Especially for repeated conventional strikes, range/payload capability is critical for delivering a useful conventional round at distances that will allow launch platform standoff well out of harm's way. However, precision targeting may allow substitution of a substantially lighter conventional warhead than the current Bullpup version, allowing longer range out of the same basic missile. On the other hand, in a conventional contingency, there may be relatively little prior suppression of Soviet air defenses. Penetration probability may be a particular problem, and low-altitude attack may not be enough to maintain it. Taking advantage of stealth may therefore be most important for the conventional mission.

CONCLUSIONS

- Very high range-independent accuracy is possible but may be expensive, especially if retrofitted into only a fraction of our missiles and considering the external support costs.

- At the moment, only a small, albeit important, fraction of targets clearly justify super precision. However, current targeting analysis is not sufficiently fine-grained to test the utility of the accuracies now feasible. Improved targeting analysis must have highest priority in the near term to support an informed decision on improved guidance.

- In the meantime, for the bulk of targets, CEP alone is not a good measure for comparing alternate long-range missile guidance schemes. Of equal or more importance are:

  -- robustness and reliability

  -- speed of and adaptability for adding new targets

  -- costs and characteristics (including survivability) of necessary C^3I support systems.

- Follow-on conventional SLCMs and ALCMS with adequate range/payload capability to deliver a

useful warhead to deep targets from safe standoff distances are required.

-- Such conventional missiles, used against the Warsaw Pact, will have to penetrate against much less attrited air defenses than their nuclear counterparts

-- A conventional ALCM on a long-range bomber and/or an at-sea reloadable SLCM would be particularly attractive in terms of munitions flexibility and sustained firing rate.

APPENDIX F

AIRPLANE AND AIRBORNE WEAPONS

Dr. Hans Mark

The coming decade will see significant advances in aviation. There are at least four technologies described in Appendix B of this report that bear on the development of new and more capable aircraft and airborne weapons. To turn the new knowledge in these major technological areas into something useful depends on an assessment of how aircraft based on these new technologies might be employed for tactical as well as strategic purposes. It is the purpose of this appendix to provide the necessary information to make such judgments.

The major technological areas most promising for advances in aviation are materials, aerodynamics, energy as applied to propulsion, and electronics. In the area of materials, there is good reason to believe that tailored materials will find major applications. Light weight, for instance, is always a requirement in aircraft construction. Good thermal properties are also important. There are, however, more exotic applications on the horizon having to do, for example, with materials that have anisotropic properties matched to the stresses and strains they experience when used as aircraft structural components. Many of the currently available composite materials already have properties of this kind and more developments in this area can be expected. Another most interesting feature of tailored materials is the incorporation of unusual electronic and electrical properties. Such materials have already found applications in the production of low observables aircraft, and further developments of this kind are to be expected as well.

In aerodynamics, there has been a genuine breakthrough in the past decade as supercomputers have made it possible to calculate flow fields with much better accuracy than has ever been possible. As a consequence, the flow around very complex aerodynamic shapes can now be accurately modeled, and results that have predictive value can be obtained. Supercomputers have indeed become the numerical windtunnels that were only imagined in the early 1970s. There have also been important increases in theoretical understanding, especially those dealing with turbulence and other chaotic phenomena. Computers have been able to develop numerical solutions for the highly nonlinear equations that describe aerodynamics (the various approximations of the Boltzmann equation including the equation of Navier and Stokes). It is interesting that these computer solutions have led to unsuspected regularities that are just now being understood. There is good reason to believe that new predictive methods dealing with highly turbulent flows will be developed in the coming years.

New technical advances in the field of energy production related to propulsion will also have most important consequences. The use of conventional hydro-carbon fuels in aircraft has reached a high degree of perfection with exceedingly efficient turbojet and turbo-prop engines. There is on the horizon, however, the possibility of burning hydrogen directly and thereby gaining as much as a factor of 50 percent in the efficiency of the engine. What makes this possible is the technology that has been developed to handle large quantities of liquid hydrogen for space flight. The development of a new generation of turbine engines or hybrid turbine-ramjet engines that burn hydrogen as the primary fuel should be anticipated.

Advances in electronics will also have an important impact on aviation in the coming years. Smaller computers have always been useful in airplanes, and this is expected to continue since computers are still expected to become smaller. Sensor technology is equally important, and these together should make it possible to improve navigation significantly so that position determinations to an accuracy of a few feet of even very rapidly moving objects should be possible. There are obvious implications for tactical applications, such as targeting, that would accrue if these advances could be turned into practical technology.

The application of new knowledge outlined for these four major technological areas will lead to new types of aircraft. In discussing these it is useful to divide the time horizon into the near term, which is defined as the next 5 years and the far term, which is defined as the next 15 years. Beyond that, it is not possible to say anything that is really useful in terms of practical airplanes. Three new types of airplanes that will be available in the near term--low-observables aircraft Vertical Takeoff and Landing (VTOL) and Short Takeoff and Landing (STOL) aircraft, and Remotely Piloted Vehicles (RPVs)--will be discussed. Three other types of aircraft are likely to be available in the longer term: long-duration patrol aircraft, heavy lift cargo and tanker aircraft, and the Aerospace Plane/SR71 follow-on. These will also be dealt with in this appendix. The six types of aircraft that have been defined are being fielded or may be developed because of the advances made in the four technological areas that have been described.

SPECIFIC DESCRIPTIONS OF NEW AIRCRAFT (NEAR TERM)

Low Observables Aircraft

Aircraft that have very small radar cross sections and that also have low visual and infrared signatures are becoming

available. This development is one of the important technical contributions made during the 1970s. Before these aircraft can be usefully employed in the field, much testing will be necessary. Indeed, it is likely that the really important questions will be: How such aircraft can be used most effectively in the military sense? What weapons will these aircraft carry? Will the weapons compromise the low-observable properties of the aircraft? What are the tactical and strategic doctrines of employment for low-observables aircraft? These are extremely important questions that must be answered before low-observable aircraft of the kind that are now being fielded become militarily useful. Edward Luttwak, in a recent book on strategy, has stressed the importance of such field tests whenever a new technology is introduced. Unless this is done properly, it may very well be that the originally expected tactical advantages of the new technology do not materialize. Having made the investment in low-observables technology, it is now most important to learn how to apply it with maximum effect. (Because of the level of classification associated with this technology, not much more can be said in a public document.)

Vertical Takeoff and Landing and Short Takeoff and Landing Aircraft

There are, of course, thousands of helicopters in service already that are genuine VTOL airplanes. However, this discussion deals with new VTOL and STOL concepts that have much better capabilities in terms of speed, range, and payload than do helicopters. Several types of these new VTOL and STOL aircraft are already in existence. Furthermore, there are plans to produce them in quantity so that they will be coming into the inventory in large numbers during the 1990s. The creation of a new generation of VTOL and STOL aircraft is another consequence of technology developments accomplished in the 1970s. Specifically, the three new aircraft coming into the inventory are the McDonnell Douglas-British Aerospace AV-8B, a fighter bomber with vertical takeoff and landing capability; the Bell V-22 Osprey, a small transport based on the tilt rotor VTOL principle; and the McDonnell Douglas C-17, a large transport aircraft that has short takeoff and landing properties.

In the case of AV-8B, much is already known about its combat capabilities, since it is a derivative of the British-Aerospace Harrier aircraft that has been in operation for many years and saw action during the Falkland Islands War in 1983. The hope is to use these aircraft to create a tactical air force that is relatively independent of elaborate ground-based facilities.

However, the value of this independence is not recognized today. Very probably this has to do with the combat experience of almost all of the leaders of the modern U.S. Air Force. They

fought in Korea and in Vietnam, and during both these conflicts, our air forces operated from airfields that were protected by political sanctuary agreements. While some of these agreements were tacit, they were nevertheless observed. Therefore, our combat leaders have little or no experience operating grim airfields that are under attack and are, therefore, likely to undervalue aircraft that could survive and operate without air bases. Our naval aviators have had similar experiences. Both in the Korean and Vietnamese Wars, our aircraft carriers were never attacked. Thus, the leaders of our naval aviation establishment have no direct experience in the operation of combat aircraft from ships that are under attack.

The extremely large aircraft carriers the Navy now operates were built to accommodate high performance aircraft. There are many people who believe that these large ships are vulnerable in spite of the enormous effort that has been made to provide defensive systems for them. The existence of an effective VTOL fighter aircraft would permit the Navy to operate such airplanes from a great many different kinds of ships. In the Falkland Islands War, Harrier aircraft actually took off from the decks of cargo ships. It is no longer necessary with such airplanes to design special ships to handle aircraft. The offensive striking power of a modern carrier task force is remarkably small compared to its cost. Right now the offensive power of a large U.S. carrier task force is lodged in 24 Grumann F-14s and everything else in the task force is there to defend that relatively small strike force. The use of a large number of VTOL aircraft such as the AV-8B on many different types of ships might correct this situation by making more aircraft available for offensive combat missions.

The Bell V-22 Osprey tilt-rotor transport aircraft is the culmination of a long development process. Application of the tilt-rotor concept provides an airplane with the VTOL capability of a helicopter and, in forward flight, the performance of an efficient turbo-prop aircraft. This is accomplished by tilting the axis of the rotors (along with the engines mounted on the wing tips) from the vertical during takeoff to the horizontal during forward flight. The successful development of the Bell XV-15 experimental tilt-rotor aircraft during the 1970s was possible because of advances in materials and control technology. The utility of this VTOL concept for a variety of purposes has been recognized by the Marine Corps (for ship home assault transport aircraft) and the Navy (for shipborne antisubmarine warfare [ASW] aircraft and for carrier on-board delivery aircraft). One thousand V-22 Osprey aircraft are now being built. Once these are available, many field exercises will be needed to explore their tactical utility.

The existence of the McDonnell-Douglas C-17 represents a considerable enhancement of air transport capability, especially in local theater situations. The long range of the C-17 will be particularly useful because it will allow direct delivery of troops and equipment to unprepared airstrips overseas directly from the continental United States.

There is another application for STOL transport aircraft that should be very seriously considered. There is no doubt that the United States will have fewer bases overseas 10 years from now than it has today. This means that refueling capabilities have to be enhanced in order to make our air forces more independent of overseas bases. One way to do this is to operate STOL-capable Lockheed C-130-type tanker aircraft from ships (a test conducted about 15 years ago proved that a C-130 can indeed takeoff and land on a large aircraft carrier). These aircraft would be operated from large converted oil tankers that could be placed anywhere in the world. The C-130 type aircraft could then refuel any group of airplanes traveling overseas from the continental United States to an operational area by locating the refueling ships in the appropriate places. This is one of many possible applications of STOL technology.

All of these developments are potentially extremely important and should be adapted by the appropriate military services.

Remotely Piloted Vehicles

The most important application of remotely piloted vehicles is, of course, weapons delivery. Cruise missiles, unmanned vehicles with an automatic pilot, are already in existence and it is most important now to vigorously explore their tactical and strategic utility.

Other remotely piloted vehicles carrying out specialized functions are also in existence. Of these functions, the most important are probably reconnaissance and targeting. There are likely to be important advances in the technologies that are relevant to the development of better remotely piloted vehicles. Specifically, these are related to better sensors and smaller computers, which will make smaller and more effective RPVs possible. In the case of remotely piloted vehicles as well, it is probable that the development of tactical and strategic doctrines for their employment should have first priority. Once the right ideas for employment are developed, the proper technical development can be based on them. (Because of the classification of many of these concepts, not much more can be said here.)

SPECIFIC DESCRIPTION OF NEW AIRCRAFT TYPES (FAR TERM)

In the longer term, the following kinds of new aircraft can be anticipated:

Long-Duration Patrol Aircraft

The Lockheed P-3 is now the mainstay of naval patrol aircraft, and it is the patrol of the oceans that would be the primary function of new long-duration patrol aircraft. The flight of the "Voyager" around the world has graphically demonstrated some of the technical advances that have made long-duration flight possible. One can anticipate that patrol airplanes with an endurance of several days are on the technical horizon. These would be large airplanes flying at relatively high altitudes at relatively slow speed. They probably would be propelled by highly efficient turbo-prop engines (probably the closest thing to such an aircraft in existence today is the Soviet TU-95 Bear Patrol Airplane). The principal mission of these airplanes would be antisubmarine warfare, and they would carry the normal complement of weapons for this function.

Long-duration patrol aircraft could also carry other weapons. It has been suggested that if they were armed with efficient kinetic energy kill rockets or perhaps with high energy lasers they could also be employed to shoot down sea-launched ballistic missiles (SLBMs). (A project to demonstrate how high energy lasers can be operated on airplanes was carried out by the Air Force in the 1970s. It was called the Airborne Laser Laboratory and consisted of a large laser mounted in a KC-135 aircraft. At the end of the program, the laser was used to shoot down five sidewinder missiles fired on trajectories close to the aircraft.) A capability of this kind might help to neutralize the Soviet submarine threat. It may be especially important to do something like this now because the new quiet submarines recently deployed by the Soviets will make conventional ASW more difficult. The aircraft would then operate as part of a strategic defense system.

There are, of course, other applications for long-duration airplanes of this kind, such as the carrying of cruise missiles or their employment as tankers. These should be thoroughly explored.

Heavy Lift Cargo/Tanker Aircraft

The largest transport aircraft operating today have gross takeoff weights of approximately three-quarters of a million pounds. The technologies to envision much larger airplanes are on the horizon; these technologies include materials, propulsion,

and control. For instance, very large seaplanes with gross takeoff weights in the region of 2 to 3 million pounds would probably be the first candidates for the hydrogen fuel option that has been mentioned. A very large airplane can carry the low density hydrogen fuel and keep it cold much more easily than a smaller airplane where both payload and volume are limited. The higher efficiency of hydrogen-burning engines would be most useful in this application. The Soviets have done some work on very large aircraft that fly very close to the surface of the sea which may be heavy lift cargo/tanker aircraft of this kind.

The existence of a fleet of such large transport airplanes would add flexibility to the military logistics system. They would be extremely important because overseas bases are less likely to be accessible in the future than they are now. Finally, aircraft of this kind could have very important applications as tankers. The operation executed by the British during the Falkland Islands War is instructive in this respect. They staged a flight of a Vulcan strategic bomber from Ascension Island to bomb Port Stanley on West Falkland Island. The total distance flown on the round trip was over 4,000 miles. The British had to employ a total of 17 Victor tanker aircraft to stage the mission. With larger tanker aircraft, fewer tankers could have done the job and the risk of failure would have been smaller.

Aerospace Plane/SR-71 Follow On

President Reagan made a commitment to build the Aerospace Plane in his 1986 State of the Union speech. Technologically, this is a very demanding proposition. The creation of a successful Aerospace Plane would draw on all of the technologies that have been mentioned. A step toward the development of the Aerospace Plane might be the development of a less capable aircraft to do the mission now done by the Lockheed SR-71, but significantly better than the SR-71. Such an airplane might be very useful for wartime reconnaissance, and it would supplement our reconnaissance satellites in peacetime. In wartime, the satellites are likely to be destroyed by Soviet antisatellite measures. Airplanes of the kind described here might very well be good substitutes. Overflight is possible in war but not in peace, and this is an important consideration. Building an SR-71 follow-on airplane using new technology should be traded off in terms of cost and effectiveness against the construction of a new, hardened satellite system.

A SPECIAL TOPIC IN AVIATION

Fifty years ago, the distinguished British mathematician, Professor Frederick William Lanchester, developed a relationship that has since become known as Lanchester's Law. Lanchester's Law says that under certain conditions, the measure of combat effectiveness of a military force increases as the square of the number of the units (people, aircraft, tanks, ships) involved and only linearly as the quality of the unit. ("Quality" here is determined in terms of speed, firepower, armor and other considerations of this kind.) One can quarrel with the assumptions made by Professor Lanchester to derive this theorem, but the basic premise that numbers are important in warfare is certainly correct. The power law is probably also correct under many circumstances, and it is for this reason that we need to be concerned about how business is done in the United States today. The quantity of airplanes that fielded today in our tactical air forces is much too small. Except for heavy bombers, the Soviets outnumber the U.S. in every other category of combat aircraft.

American combat air forces generally have only small numbers of aircraft because the unit cost of airplanes is so high. A fighter aircraft such as a Grumann F-14 fully equipped today costs over $30 million, which is a factor of over 100 more than the cost of a similar airplane in World War II. (The Lockheed P-38 had a unit cost of less than $300,000.) Inflation in the intervening period might account for a factor of 10 but not for a factor of 100!

Why are airplanes today so expensive compared to those built in World War II? Part of the answer is the increased sophistication of current technology, but a large part of it is the very different philosophy we use to build airplanes today than was used in World War II. Combat aircraft today are built to last 20 or more years. They are constructed essentially for peacetime operations, or for situations where loss rates are predictably small. In combat, the half-life of an airplane used against similar kinds of air forces is likely to be a few hours or some days at most. That certainly is the experience, for example, in recent Arab-Israel conflicts, in which air forces were intensively employed on both sides. If airplanes in combat last only a few hours or days, why should they be constructed to survive for 20 or 25 years under peacetime conditions? What does it cost to build an airplane that can be flown for such a long period of time?

When compared to the construction techniques that were employed in World War II, we pay much more attention to details and use much more expensive materials today than we did during World War II. Would it be possible, for example, to downgrade the materials and to put together the structure and the engines of modern aircraft with less sophisticated techniques in order to

lower both airframe and engine costs? Would it be possible to use less expensive materials and less extensive testing programs but still maintain the high performance configurations of our combat aircraft? Would it be possible, in short, to build a "cheap" version of the General Dynamics F-16 and the McDonnell-Douglas F-15 using the techniques just described? The answers to these questions are not known, but perhaps it would be instructive to examine them.

Another expensive item is the electronic navigation and fire control system that is currently put on our first-line combat airplanes. Is it necessary, for example, to have the same electronic and avionics suite on <u>every</u> airplane in the fleet? Is it possible to initiate a leader-follower concept in combat that would allow one aircraft, say, in five, to have the full avionics and electronics equipment and for the other four to have less capable equipment? These other four aircraft would then play follow-the-leader in combat. What would be a reasonable estimate, for instance, of the equipment that could be eliminated from the very expensive electronic suite and still retain significant combat capability for the follower airplanes that would not have the suite? None of these questions has really ever been dealt with in detail.

The basic proposal here is to determine whether it is feasible to have two versions of the same aircraft. One would be the all up peacetime version, built to last for 20 to 25 years with all the relevant electronics, avionics and fire control systems built into the airplane. These would be the front-line, leader aircraft in war. At the same time, we would build a larger number of wartime airplanes that would have the same aerodynamic configuration as the peacetime aircraft, but would be shorter lived and would have less capable avionics electronics and fire control systems. The wartime airplanes would be considerably cheaper and would be built in larger numbers. In terms of operational employment, peacetime airplanes would be used for training and familiarization and tactical development while wartime aircraft would be flown less often but would be explained in full time training exercises, but would be flown less often but would be employed in full-time training exercises.

There is no doubt that the number of aircraft we now deploy is too small compared to the expected loss rates in intense combat. The essential question about this proposal that must be answered is how much less the inexpensive airplanes proposed here would cost to build. If the follow-the-leader principle is rigorously applied in developing combat tactics, would it be possible to reduce the cost of the wartime aircraft by as much as a factor of 2 or 3 over their peacetime equivalents? This question has never been dealt with, and our technical expertise has never really been brought to bear to look at this proposal. A factor of 2 or 3 in cost would be interesting because it would

double or perhaps even triple the combat strength of American air forces.

In considering this matter, it is important to remember that the Soviets actually do something of the kind that is proposed here. They have many more combat aircraft than we do primarily because they do not throw anything away. They are still flying Mig-17s and Mig-19s. Even though these airplanes are not as capable as the first-line aircraft they have, the Soviets believe that they could be useful in combat. If nothing else, they add to the numbers and in the confusion of combat they may significantly reduce the edge we think quality provides. In this way, the Soviets show that they have understood Lanchester's Law better than we have. They have roughly the same number of first-line combat airplanes as we do but also have a huge reservoir of old and still somewhat-capable airplanes to throw into a battle if that has to be done. There are maintenance and operations costs for doing this; however, they pay less attention to that than we do and apparently believe that the cost is worth it. At the very least we should determine whether what is proposed here is technically feasible. A study team should be put together to look at the question.